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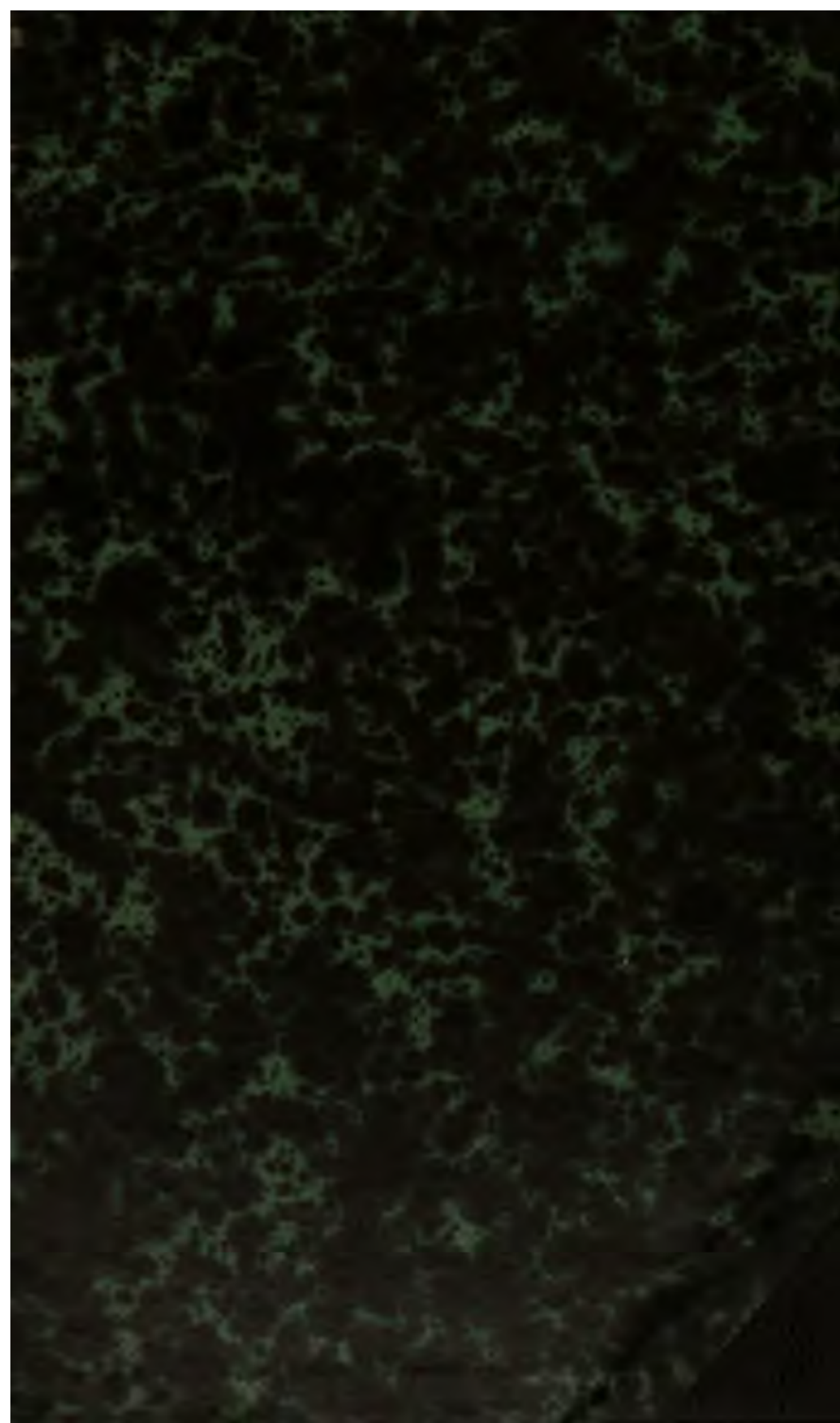
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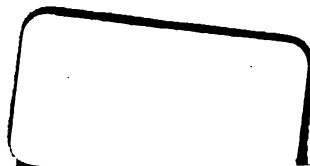
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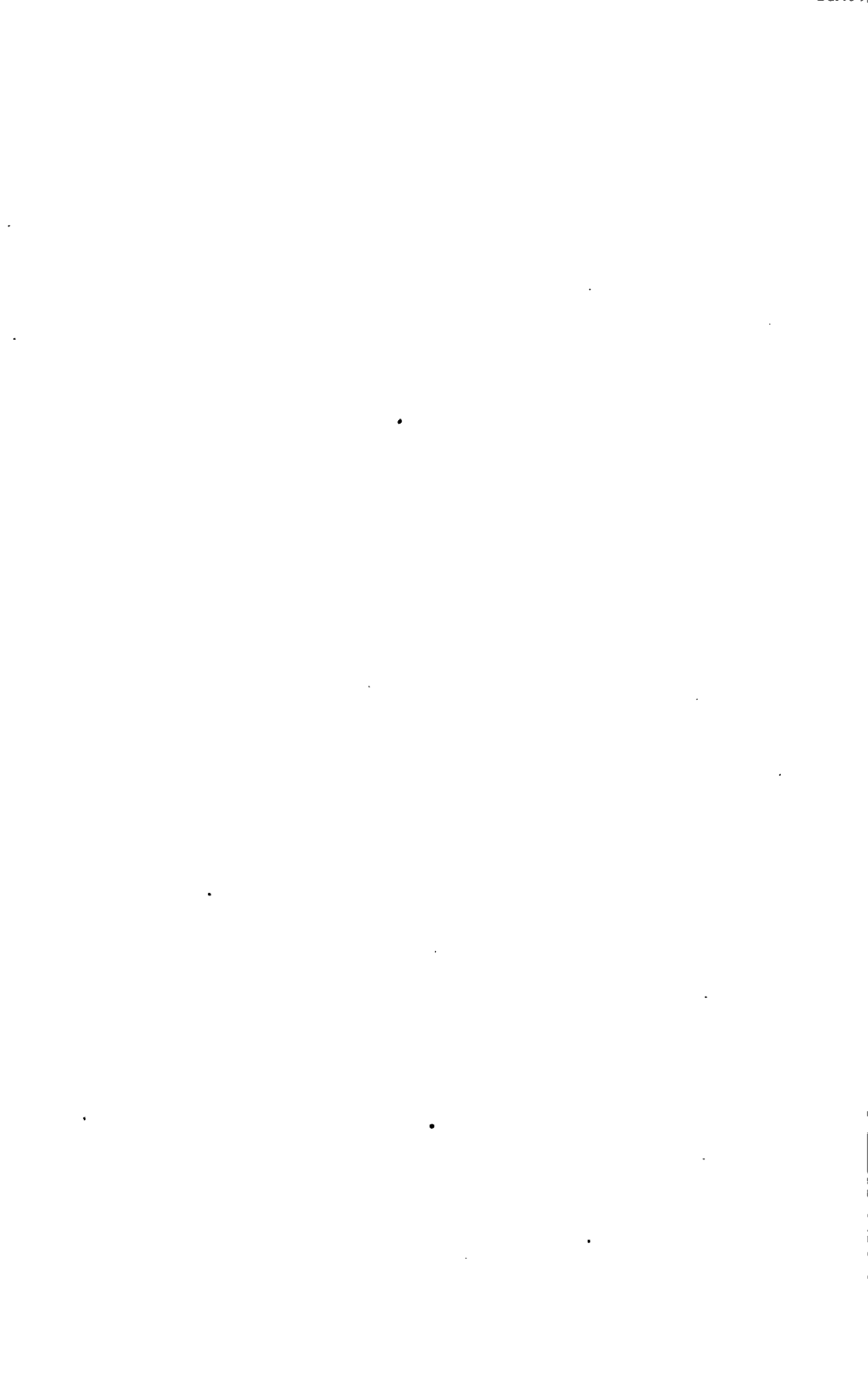


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THE  
BRITISH AND FOREIGN  
MEDICO-CHIRURGICAL  
REVIEW

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OF  
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THE  
BRITISH AND FOREIGN  
MEDICO-CHIRURGICAL REVIEW.

JULY, 1876.

Analytical and Critical Reviews.

I.—The Use of Alcohol.<sup>1</sup>

THE evil deeds of Alcohol against both the individual and society are so glaring and noisy, that orators and writers are sorely tempted to dwell on them with an exclusiveness which overshadows any inquiry into its possible virtues. In his 'Diseases of Modern Life,' Dr. Richardson piles up the usual heavy indictments against this article of diet, enumerating the various pathological conditions consequent on its excessive employment; but he does not allude to the diseases it may have warded off, the lives it may have lengthened, and the misery it may have lightened. The best he can say of it is that it is a "pleasant shroud," implying thereby that its shadow is as the shadow of death, a decent dress for a self-dug grave. We must decline to accept the unsavory association, and prefer to appropriate Sancho Panza's proverb to an use he would not condemn, "Blessed be the man who invented" alcohol, as well as

<sup>1</sup> 1. *Diseases of Modern Life*. By BENJAMIN W. RICHARDSON, M.D. London, 1876.

2. *Les Aliments d'Épargne, Alcool, &c.* Par le Dr. A. MARVAUD. Paris, 1874.

3. *On the Issue of a Spirit Ration during the Ashanti Campaign of 1874*. By E. A. PARKES, M.D., Professor of Military Hygiene in the Army Medical School, &c. London, 1875.

4. *Observations on the Effect of Cuca, or Coca, the leaves of the Erythroxylon Coca*. By Sir R. CHRISTISON, Bt., M.D. 'Brit. Med. Journal,' April 29, 1876.

5. *The Non-Alcoholic Treatment of Disease*. Notes of cases treated at the London Temperance Hospital, with introductory remarks and supplementary suggestions. London, 1876.

6. *De l'Alcoolisme*. Par le Dr. V. MAGNAN. Paris, 1874.

7. *Alcoolisme*. Par Dr. E. LANCEREAUX. 'Dictionnaire Encyclopédique de Médecine et de Chirurgie,' 1865.

"sleep; it wraps a man up like a *cloak*." Like a cloak it impedes the skill of the limbs, it smothers the voice, and its final consequence if used during exertion is increased weariness; like a cloak too it may by excess stifle us altogether, or lessen the vigour of our life. But also, like a cloak, it sweetens and renders more renewing our rest, it soothes the wear and tear of sorrow, pain, and ill humour, and after bodily or mental toil or sickness it may often make just the difference between good health and ill health, between life and death.

The most conspicuous of the primary actions of alcohol is a dynamic narcosis of the ultimate fibres of the nerves of sensation and vaso-motion—most conspicuous because exhibited in the cutaneous surface under our eyes. The sense of touch is rested by being relieved of its extreme acuteness, and the muscular tone of the capillaries is relaxed, so that they dilate, and weaken the current of blood, which congestion (like those from all other causes) is accompanied by increased frequency of cardiac contraction and of respiration. We are all familiar with the slight numbness of the upper lip and insensitiveness of the face to cold or heat quickly experienced by a temperate person after a glass of spirits, and the flushed skin and perceptible sharpness of the pulse following the narcosis.

The sluggishness of the blood-current has a corresponding effect on the mucous membranes; their endosmosis is arrested, and thus oxidation within the organism and nutrition are retarded in proportion to the dose of the agent. At the same time alcohol turns aside to its own profit and conversion some of the oxygen contained in the body, and thus spares the tissues from the destructive assimilation to which this oxygen would otherwise have subjected them. It may in this sense be fairly called a "saving food"—*un aliment d'épargne*.

As part of the alcohol disappears by oxidation in the body, it is in some sense partly a calorific agent; but then the increase of temperature so originated is not enough to outweigh the loss determined by that which remains unconverted in the blood; so that there is a decided tendency to lose animal heat evinced after the ingestion of distilled liquors; which indeed is very marked if the quantity be large, though the anæsthesia prevents its being recognized by the sufferer.

Now, few probably will deny that there are circumstances, by no means of unfrequent occurrence, when it must contribute to the well being of the individual to modify the nutrition and other vital acts of the body in the directions indicated above. But a great many will dissent from the opinion that it is wise to employ the means thus placed at our disposal. They say you are paying too much for your whistle, that the retardation of the

blood-current and the relaxation of the capillaries necessarily tend to permanent organic lesions, latent indeed but insidious, and aggravated by each additional dose in proportion to its frequency. The risk that these histological changes should be permanent, or at least that each repetition of them should leave permanent deterioration behind it appears to them very great; it appears to Dr. Richardson so great, that he affirms the only way to avoid the probability of shortening one's life by alcohol is to abstain from alcohol wholly and entirely; to sacrifice altogether the blunting of pain, the calming of anxiety, the refreshment after toil, the softening of sleep, the renewal of the appetite, the gladdening of the heart, the smoothing of the temper, which the temperate man day after day puts to the credit of fermented drinks. The ease is a phantom profit; it vanishes like fairy gold and leaves an enhanced heap of pains behind. To render the sufferer insensible to this accumulating sorrow a larger and a larger dose is daily requisite, or else an amount of self-denial has to be exercised which is in itself a serious evil. Such is the serious allegation.

Here, beyond all question, lies the main matter to be decided by the physiologist. Is the risk a real risk? Are the chemical changes in the flesh and blood wrought by alcohol permanent or dynamic? With those who hold that dynamic or functional acts do not exist, this is not the place to argue—they may fairly demand more space than is possible here to have their doubts dealt with. But conceding that there are certain processes which affect the body, especially the nervous system, for a time, and then leave it exactly in the same state it was before, with the same expectation of life, so far as the said process is concerned, then the initial phenomena produced by alcohol may reasonably be classed among such processes. Experience seems to prove that its action may prevent the destructive agencies of a rugged life from paining and injuring the body for a time, and that during the respite thus obtained the tissues may be renewed by rest more completely than had no alcohol been taken. Appetite and vigour are restored, and the languor due to the exhaustion of labour is not felt. The reactionary languor which certainly follows excess is never consequent on moderate doses. The blessing does its quiet duty, and nothing more is known of it. A vast army of respectable working men of all classes, from the peer to the peasant, take their allowance of alcohol at one or two principal meals daily from youth up to extreme old age, without the slightest wish ever to increase its quantity or strength.

Everybody knows too well that there are also many to whom moderation is a distasteful state, and to whom the only beauty



of alcohol lies in its abuse. To them Dr. Richardson's is truly a sage warning, and we sincerely hope that the sadly true picture he draws in burning words of the fatal effects of increasing the dose of alcohol in obedience to the morbid craving of a vitiated taste, will be read by all who have ever experienced this temptation. Let each one strictly examine his own conscience, and if he finds that the more he drinks the more he wants to drink, if the memory of the moments of intoxication is attractive to him, let him forswear for ever the use of alcohol, for he will not be able to shun the abuse. But do not let him insist upon his example being followed by others, whose self-restraint allows them freely to enjoy the gifts of nature without risk. And let him not denounce as traitors to the cause of temperance those who try by observation to find out and define what are the essential qualities which have so long caused alcohol to be valued by the human race, and have caused it to be valued in spite of its acknowledged evils.

An opportunity of rare value for obtaining answers to physiological and social questions is often afforded by that concentration of healthy men under strict discipline and observation which we find in an army on active service. Such an opportunity has been seized by the Professor (alas! we must say the *late* Professor) of Military Hygiene at Netley, in the report which he made for the War Office on the experience of those who marched through the dashing Ashanti campaign, concerning the effects of the spirituous liquors issued to them. The report is grounded on the evidence of all sorts of men, old and young surgeons who like their glass, surgeons who are habitually total abstainers, soldiers selected from the same mixed classes, sergeants from the hospital corps, and combatants from the ranks. The agreement of evidence is very remarkable, the variations being almost entirely in the degree of the effects stated to be produced.

The experiment was made on the daily consumption of rum in moderate doses, not exceeding half a gill (two and a half fluid ounces), and generally diluted as punch or put into tea.

The results are as follows :

1. Entire absence from alcohol did not make the men who abstained more sickly as a whole, or more disposed to malarious fever, nor did it diminish the average marching powers of those who had used themselves to it.

2. The evidence is against the usefulness of alcohol during physical exertion. If rum is taken during a march its invigorating effects rapidly go off, and are succeeded by a languor greater than that relieved by it. The evidence for this point is

taken from Indian experience, compared with that derived from the Ashanti campaign.

3. Taken after the physical exertion is over, at the end of the day's march, alcohol is equally reviving as if it were taken earlier. And it is equally reviving to those unaccustomed to its use as to its habitual employers.

4. If this revival is followed by sleep no reactionary languor is perceived, and the refreshment is more complete. Some of the men thought they marched better the morning after rum had been issued than when it had been withheld by accident the evening before.

5. The digestion and appetite are perceptibly improved by this moderate use of spirits.

6. Alcohol is more useful in a hot moist atmosphere than in a dry cold one. The experience of Sir Garnet Wolseley's famous Red River expedition was that hardships were best borne, or at least equally well borne, without alcohol; and many of the officers went out to the Gold Coast with this impression. But they returned with their faith much shaken. It seemed to many as if alcohol were the only thing which could enable them to do their work, and that the instinctive craving for it experienced by Europeans after labour in a tropical climate is justified by reason.

This demand for alcohol arises probably not so much from the heat of the sun as from the saturation of the air with moisture. On the coast the mean difference between the dry and wet bulb is only  $2.5^{\circ}$ , and in the interior the air is often quite saturated. Consequently the perspiration from the surface of the body is not carried off. On the slightest exertion the skin is bathed in sweat, not because more is formed, but because less is evaporated. The total temperature, therefore, rises, and a depression is felt like that which accompanies over-heated blood in fever, in sunstroke, in hyperpyrexia, and rheumatism. As in fever, so here, cooling and relief are obtained by the use of spirituous liquors, and memory suggests a recurrence to the same relief on similar occasions.

In this series of observations we have traced out two of the uses of alcohol in active life—one analogous to the use of sleep, namely, to stay the continuity of the destructive assimilation set on foot by labour, to rest the tissues from over-rapid life, and to give them time to lay up fresh force; the other defensive against the depressing influences of cosmic agents, by blunting the nervous system to them. Both of these uses are in health preventives of disease, and in disease are curative in a more conspicuous manner, inasmuch as the deleterious dis-

turbances of vital processes against which they contend are more grave.

But it is brought out very strongly that alcohol does not impart force, or enable extra work to be done without injury to the body. If taken during exertion it rather entails more weariness in the end by blunting the sensation for a time to the wear of the tissues, which consequently go on being worn out in a reckless manner. Its only legitimate employment during labour is just before the end—say during the last mile or two of a hard march—when its immediate anæsthetic effects will not have time to be worked off before repose is afforded, or when the atmospheric influences are peculiarly depressing.

Physiologists cannot too strongly impress these facts upon the public, for it is doubtless from the bad results of its employment during work that the moderate consumption of alcohol has acquired its evil fame. It is quite true that we frequently meet with most respectable persons, both male and female, who have never been drunk in their lives, yet have lapsed into a condition of alcoholism by taking extremely small doses of stimulant between meals, to enable them, as they say, to bear up against their work. The same quantities added together and consumed once a day after work would probably have secured them a good night's refreshing rest and an appetite for the morrow. And it is quite true that these people have more difficulty than drunkards have in surrendering their habits and substituting temperance; and, indeed, for them a total abstinence from alcohol for many months is usually the only course which prudence can recommend.

Dr. Parkes has not bounded his inquiries to the establishment of the principle that alcohol is of little service as a stimulant for obtaining the full safe amount of labour from the animal frame; he has sought for something to take its place by really doing what it professes to do. Is there no better way of supplying in a convenient form what may renew the strength during exertion without entailing future injury? A few years ago our military neighbours would have promptly answered, "Bouillon." But the kettle and the can are terrible impediments to rapid movement, and the delays arising from cooking appear to have contributed in no slight degree to the unhappy termination of the Franco-German war. What is wanted is something of small bulk and easy carriage, facile of digestion when mixed with hot or cold water, agreeable to the senses, and not quickly spoilt, and which would not supersede the usual food, but rather increase the appetite for it when the opportunity for meals is afforded.

Dr. Parkes considers that he has found this desideratum in

the meat extracts to which manufacturing industry is now turning its earnest attention. In order to test this opinion he made experiments on three intelligent soldiers, whom he caused to march in "heavy marching order"—that is carrying weight to the amount of fifty-one pounds—twenty miles a day for six days, taking, in addition to their usual rations, on two days rum, on two days meat extract, and on two days coffee, all in moderate quantities. These several stimulants were given at 12.30 and 2.30 during the march, and at the end of the experiments the men were asked to state their candid opinion of their relative value in imparting force. All three awarded the palm to the meat extract under ordinary circumstances; "it does not put a spurt into you for a few miles only, but has a lasting effect; if I were ordered for continuous marching, and had my choice, I would certainly take the meat extract." At the same time it was thought that during wet weather the rum was more warming, and the coffee quenched thirst best, and gave a more comfortable feeling than the other two. No decided effect of the rum, extract, or coffee, could be traced in the pulse and temperature; the exercise appeared to over-ride all other conditions; and if the hearts had got quickened by the stimulants they had resumed the pace natural during exercise by the time of observation.

Appropos of this last matter Dr. Parkes gives a table, showing the remarkable effect on the heart of taking off the accoutrements and lying down for only sixty seconds. In one of the men who was weakly and evidently overtaken by the exertion, the pulse fell on the average no less than 43 beats in the minute; in another it fell 39 beats, and in a third 16 beats. The observation accords with the experience of professional pedestrians of the high value of even the shortest period of untrammelled rest at frequent intervals. To take off the clothes and be rubbed down in the horizontal posture seems to make them as fresh as at the commencement of the walk.

We must not limit the sustaining value of nutritive substances to animal food in a rapidly absorbable form. Purely vegetable matter, of mixed starchy and nitrogenous nature, may be so prepared as to be readily absorbed even by a frame exhausted during labour, and in this state seems capable of acting as an extraordinary supply during extraordinary demand. Here is an experiment in the leisure and ease of peace, instead of under the cruel pressure of military necessities. In the summer of 1872 a remarkable feat was performed by those warriors against time and space, the civil engineers. It was necessary to shift the rails on upwards of 500 miles of permanent way on the Great Western line from the broad to the narrow gauge,

and there was only a fortnight to do it in. The work to be got through was enormous, for the Great Western is one of the few English lines whose rails are held down by bolts screwed into nuts. All these had to be unscrewed and replaced after moving the heavy rail two feet, a very different operation to prizing out a spike, shifting a rail a few inches, and hammering in a spike again. About 3000 men were employed, and they worked double time, sometimes from four in the morning till nine at night. Gallantly they won the day, not a soul was sick, sorry, or drunk, and all was ready for a down train due on Saturday evening, June 20th. Now, what does the reader guess was the extraordinary support of this wonderful spurt of muscular energy? —weak SKILLY! To spare every ounce of strength, the men were hutted along the line, and brought with them bacon, bread, cheese, cocoa, &c., to provide their usual meals at usual times, but no beer, spirits, or alcoholic drink in any form. A pound and a half of oatmeal and half a pound of sugar were allowed to each man daily, and to every gang of twenty-one a cook was told off. The first thing done in the morning was to breakfast, and then the cook and his caldron started along the line till water was found convenient, and a fireplace of stones was built and the pot boiled. Oatmeal was then sprinkled into it with sugar, and thoroughly well boiled until their gruel was made. As soon as the shout for drink was heard, buckets were filled and carried round with small pannikins to convey it to the panting mouths. The men liked it exceedingly, and learned by experience the importance of having it well cooked. How Hippocrates would rejoice to see the precious oatmeal gruel, which he thought so unique a specific for tiding fever-smitten patients over their dangers, employed to shield the Herculean frames of British engineers against the equal dangers of fierce toil. With what racy aphorisms he would regale the cooks, telling them that more than 2000 years ago he had written a treatise on the use of this article, had repelled with scorn the foolish contempt of the public for simple things, and had thought this despised gruel of such paramount importance, that he, the companion of kings, statesmen, and philosophers, would spend his time in telling them how to make it good, how to boil it till it would swell no longer (so that it might not swell any more in the stomach), how to strain it and settle it. And this valuable bit of cookery (the only one in his works) the world has been reading all this time, and is only just beginning to profit by. It was as clear to him as it is to our advanced physiologists that excessive muscular exertion produces the same effects as continued fever (*ἐς πυρετὸν καθίσταται μακρότερον*), its chief dangers lying in rise of temperature and arrested cutaneous action; and

that its "antagonist" is nutriment capable of rapid absorption, dissolved in the most essential nutriment of all, water. And he would have suggested that after extraordinary exertion the best safeguards are a bath, wine, and a good dinner with dilute drinks, in the order here translated (ἐκ τοῦ λουτροῦ μαλακὸν οἶνον, δειπνεῖν ὡς πλείστα καὶ παντόδαπα σίτια καὶ ποτῶ ὑδαρεῖ).

Experiments on the use of the Cuca leaf by scientific persons are as yet too few and recent to justify a judgment on its final effect on the metamorphosis of the body and nervous system. The immediate action, according to the observations of Sir R. Christison on his own person, is to diminish in a very marked degree the excretion of solids, that is mainly of urea from the kidneys, and therefore to spare the wear and tear of the muscles. To guess what its contemporaneous effect on the nervous tissue is, requires a further analysis of the amount of phosphates lost. Its subjective influence is to remove in a remarkable way the sense of fatigue and to postpone the craving for food, without affecting the powers of digestion and sleep, at least when taken in the temperate fashion adopted by the venerable and prudent professor. Thus used it appears to offer a valuable contribution to the dietary of economic industry.

Preventive and curative medicine has become so entirely one that it is impossible to separate the consideration of the use of alcohol as an article of daily diet from that of its employment in therapeutics. The requirements of civilized industry and the action of disease agree in this, that they tend to exhaust the nervous system beyond the point which our instinctive sensations tell us is consistent with perfect health. If alcohol be a defence in the one case, as we think it is, it will be a defence in the other. If after labour it prevents the current of destructive assimilation from running on with a moulting faster than the exhausted forces can remedy by constructive renewal, then in disease it can do the same. In typhus, for example, when the flesh and blood are poisoned, it is probably better that renewal should be in abeyance, for it would be chaotic and unnatural. But we know by the amount of urea and phosphates in the urine and by the fecal excretion, that the muscles and nerves are melting away as fast as if the bedridden patient were scaling Mont Blanc with nothing to eat. To a certain extent and at certain stages this happens in all diseases. Except real hypertrophy, there is probably no disease in which, if we knew exactly when to introduce it, alcohol would not be beneficial. Our ignorance stands in our way: though "fools will rush in" with their foaming tankards at all seasons, and then point with complacency to the cases in which they chance to be right;

"angels fear to tread" where they are conscious that harm may be done as well as good.

We would suggest as a guiding light to the administration of nutriment and of alcohol in disease the result of observation on the appropriate periods of their use in respect of active exertion. And we would call attention also to the forms in which they should be presented to the stomach. At the commencement and during the anabasis of the acute morbid symptoms it is probably possible to introduce with advantage food, and it is not desirable to check the moulting of the tissues, which are to a certain extent poisoned by the morbid process. Alcohol stays it for a time, to return with more injurious effect later, like rum taken by a tired man during his march. But when the moulting begins to damage the integrity of the nervous system and muscular fibre, of which (for example) a warning is given in typhus by tremor and delirium, then alcohol acts as a saving drag to the downward course. If its use is begun just at the right hour it inaugurates convalescence. A knowledge of the natural periods of febrile actions will come in well here, and with that it may be employed just at the crisis with great advantage. During convalescence also there are few cases where its use is not applicable. Again in chronic cases it may be used to protract life or strength till the local derangements are overcome by the general vitality of uninjured functions. It would infringe too much on these pages and the patience of the reader to offer many similar illustrations which are obvious to every pathologist.

It may be remarked that the rest afforded by alcohol may learn from the rest which nature gives, to be periodical. When periodical it is most effectual with least risk. Like sleep, it should come but once, or at most twice, in the twenty-four hours; and, like sleep, it should not be taken in snatches, but in a good and sufficient dose. Let not brandy be dribbled in by drops by the nurse when she is so disposed, but in one or two draughts daily.

As to form, let the stimulant be fairly dilute, and also agreeable, or else that gladdening of the heart, which contributes so much to its usefulness in health, is lost in disease. Diluteness is also a virtue in the nutriment, whether it be farinaceous, as Hippocrates preferred, or animal, as Dr. Graves taught. In the administration of this last-named article of diet periodicity is of no importance, and it is better that a continuous reception should be kept up in order that the favourable accidental times when the absorbents are pervious should not be passed over.

We are inclined to think that more advantage will accrue from the application to medicine of the knowledge gained from

physiology than from a rough crucial experiment of treating a continuous series of patients without alcohol altogether, and then comparing them with a corresponding series treated in the usual fashion. Nevertheless, such a trial is not without considerable value, and we look therefore with satisfaction upon a report made by Dr. Edmunds of sixty-five cases treated at the London "Temperance" Hospital. This establishment was opened in 1873 for the very purpose of putting the question to the test of clinical experience, and alcohol in any form is denied to the patients; even tinctures being superseded by glycerine solutions. And it may be remarked that the medical officers are not *ex officio* total abstainers in their own persons, but are simply so far convinced of the needlessness of the large quantity of fermented and spirituous drink consumed in general hospitals that they are willing to superintend the experiment, the opportunity for which is given by the benevolence of some wealthy enthusiasts; and we presume that when they saw the experiment failing, they would transfer the patient elsewhere, or else break the rules of the institution. So that no serious risk of danger to human life is incurred. Of course it is not pretended that these few selected cases prove anything; many thousands must be carefully analysed before a guess at a result can be hazarded; but they are at all events a first step. Of the sixty-five more than two-thirds appear to be abstainers on principle, and of the remaining twenty it is very doubtful if more than half a dozen would have had alcohol prescribed for them at any hospital—indeed to the majority it would have been expressly denied—so that no comparison can be instituted, and probably it will be many years before it is attempted. The earliest contributions from this laboratory to the therapeutic art will probably be the invention of some substitutes for alcohol under various circumstances, which should contain its virtues without its vices, in the fulfilment of the indications usually held to demand its use. This would be an aim similar to that of Dr. Parkes, who was not content merely to find fault with alcohol as a stimulant to exertion, but endeavoured to supply its place with something better in the shape of meat extract and coffee. We would call attention to efforts in this direction made by the physicians to "Temperance" Hospital.\* One is the employment of digitalis as an arterial tonic in typhoid fever, instead of the wine which delirium is held to justify. Again in a case of a dipsomaniac, sedatives and other "remedies which overcame the constant craving she had for intoxicating liquors,"

<sup>1</sup> We have taken "Temperance" for a proper name, as one might say "Guy's" or "Evelina;" otherwise it would seem to imply that other hospitals are intemperate, a pharisaism unworthy of the founders.



were given. Ammonia was administered to a total abstainer to help him to bear up against hæmorrhage during an operation. Hop tea is also suggested by Dr. Edmunds for the use of generally weak persons who often find the benefit of a glass of bitter beer at meals. Sal volatile and aromatics appear to be employed, and chloroform and chloral are not absent from the dispensary, but no statement is made in the little volume under review as to whether they are used to take the place of the more agreeable "*aliment d'épargne*."

While we look with interest at attempts to find among drugs improved substitutes for alcohol in sickness, we would protest against the careless introduction of these into the daily dietary. As Dr. Edmunds says, "those who in place of alcoholics substitute medical bitters, chlorodyne, chloral, laudanum, morphia, &c., merely fall into habits more injurious and more dangerous." We believe him to be quite right, and that the physiological action of alcohol is the wholesomest among all the agents of its class.

Though the subject of the present review of the question is the "use of alcohol," we are tempted to say a few words concerning its abuse. From the abstracts of the literature of alcoholism made by Dr. Magnan and Dr. Lancereaux it would seem that those who have to pay the penalties of excess may be roughly divided into two classes—first, those who employ alcohol as a stimulant to immediate work, and second, those to whom the physiological action is a temptation to try to increase it by an additional dose. The first class (whom one may call the "nippers") runs the greater risk of physical degeneration, though on moral grounds they incur the least blame. Their excess is a gross error of judgment, but not a delusion; and it may often be combatted in reasonable argument, either by pen, by voice, or by example. Let the results of such an experiment as that of Dr. Parkes be made clear to them, and they are led to repeat it on themselves, to substitute nutriment or temporary rest for alcohol, and to convince their minds in the only method in which minds ever are convinced—personal experience. But to talk to them as if they were drunkards, who must give up alcohol entirely when they want to be well, is revolting to their reason.

The other class is a very troublesome one to society, and is a proof of how much of the brute remains in human nature. It is subdivided by M. Trélat<sup>1</sup> into "drunkards" and "dipso-maniacs," the first being defined such as get drunk whenever

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<sup>1</sup> Trélat, 'De la Folie Lucide,' 1861, p. 151.

they have the chance, the last such as get drunk when their mania comes upon them.

For "drunkards" there is no doubt that each additional glass diminishes the self-control and self-respect which naturally arrest over-indulgence in reasonable creatures. All the lower animals—such as bees, rats, monkeys, bears—when they take alcoholic liquids at all, will go on imbibing it till they get helplessly intoxicated. But it is not so with man. Many a one, with even a considerable "survival" of the brute in his constitution, has yet such a foresight strong enough to stop when he has had enough, in spite of a secret hankering to go on. And the foresight will be strengthened by use. It is obvious, however, that he is more or less in the peril which so alarms Dr. Richardson, and the question of the more or less he himself alone can determine. If, after trial, he finds his craving irremediably that of a drunkard, if the influences of religion, of regard for others, and self-respect, are not sufficient to hold him back, total abstinence is his path of safety. But that this bestial craving for too much is the rule, and a bodily contentment with enough the exception, even among self-educated persons, we deny; and we deprecate strongly the burdening of the consciences of the majority with a restriction necessary only to a minority. Whether the drunkard's craving be "a disease" is a question of words; it is, at all events, a degradation of the typical perfection of human nature; he is a lower sort of man than the temperate, and must become rarer as the race improves under the inevitable advance of time.

What shall be done in the mean while? Shall we consider the "poor drunkard" as a patient, and using the power which, according to Aristotle, the strong and wise may justly exercise over the weak and foolish, shall we forcibly put him in a hospital for cure, as we submit a dog or a horse to treatment? There are manifold dangers involved in such a costly experiment, of the same kind as have to be guarded against so rigidly in our management of lunatics, only much more serious, inasmuch as our rights as against the person are more problematical. And again, it is quite certain that if intemperance were thus looked upon as a disease, as a misfortune rather than a fault, the sense of responsibility would be weakened, and a man would in no higher degree feel bound to be sober than he feels bound to be sane or athletic. The religious and moral pressure would be equal in respect of our attainment of all three excellencies, which would be a great misfortune for the world. Circumstances have combined to weaken the hold which, in former ages, Society had over the sense of responsibility in the semi-lunatic; it is an unavoidable result of the merciful consideration

shown for his temptations, that he makes much less effort than of old to retain his senses, and, therefore, oftener loses them. Let not the same thing happen to the weaker brethren whose case is now under discussion, so that drunkenness should ever cease to be a crime, a sin, and a reproach. Already people bestow too unmixed pity on a madman, whose loss of self-control is really worthy of serious reprobation, because it was not restrained in an early stage. We hope they will still continue to hold a drunkard entirely responsible.

The same objection lies, but to a less degree, against voluntary "retreats." Properly conducted, such institutions afford an opportunity for the habitual exceeeder to regain health and practise self-discipline. Their failures have arisen from too much being expected of them; a perfect cure has been looked for, a conversion of an intemperate man into a temperate, of him whose joy was intoxication into him who feels no pleasure in taking more than enough, and who, therefore, can be trusted with a wholesome allowance of alcohol. This is impossible. The washed sow will not trip daintily through the mire, but must be kept quite dry if she is to be kept long clean. The retreat must be followed up by total abstinence.

"Dipsomania" is indubitably a disease; it begins with premonitory symptoms, both of body and mind, headache, anorexia, gnawing at the epigastrium, depression of spirits, loss of temper, and it runs through a regular succession of stages. It is at first intermittent; the patient will for months or years feel no inclination to exceed, often no inclination to take stimulants at all. Then he breaks out into a fierce fit of uncontrollable debauchery, which, if he be not put under restraint, ends in delirium tremens. The feeling of penitence and self-disgust is terrible, and many times leads to suicide, but is not sufficiently rational to curb the raging impulse. It is itself too wild to antagonise wildness. Argument at such a time is like emptying a teacup on a burning house. When this has passed away, he is as sober and amiable as other people; but each access, if indulged in, shortens the interval, weakens the powers of control, and is more severe than the last. If the patient be kept from drink, that is not the case. The dipsomaniac is unfortunately situated. He feels his fit coming on, and would like to be locked up in an asylum, but he cannot exhibit any delusions which would entitle him to the necessary certificates, so he cannot get the advantage of external restraint till he has qualified for it by falling into the horrors he was anxious to avoid. He is certainly a fit subject for the temporary treatment of a retreat.

It is uncertain whether the habitual abuse of alcohol does

or does not lead to dipsomania. Systematic writers usually bring this accusation; but the two typical cases detailed by Dr. Magnan do not substantiate such a causation. More generally the malady appears to be a special development of hereditary insanity, and the slight inclination to drink which remains chronically between the fits would go to prove that it is distinct from ordinary drunkenness in its pathology, and consequently in some details of its treatment. The unfortunate who is liable to it will do well to remove all alcoholics; not only out of his sight, but out of his house, in order that they may not lie in his way when he is unexpectedly attacked by a paroxysm. And when he feels the premonitory symptoms he should put himself under the charge of a firm and secret friend, who will prevent the latent disease from publicly declaring itself by overt acts.

With these precautions duly observed against the dangers of excess, we hold that it is quite unnecessary to denounce the habitual use of alcohol by temperate persons. And in the cause of temperance we believe such denunciation to be injudicious from the feeling of injustice which it arouses.

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## II.—Cholera in the United States in 1873.<sup>1</sup>

In February, 1873, Cholera appeared at New Orleans, and, in the course of the next five months, extended over a considerable portion of the valley of the Mississippi and its tributaries. As soon as the disposition of the disease to spread became obvious, an effort was made to collect as much evidence regarding it as possible, from the notices in the newspapers of the various localities in which it showed itself, and from these, and some contributions by local practitioners; an account of the outbreak was compiled by Dr. A. B. Judson, of New York, and published, under the auspices of the American Public Health Association, in their volume of Reports for 1873. The same volume contained a paper by Dr. Ely McClellan, of the United States Army Medical Staff, on the epidemic as it appeared in Kentucky.

As these papers were deficient in much that was necessary to settle the questions of origin, and means by which cholera was diffused in 1873, the Senate and House of Representatives by a joint resolution, approved by the President March 25th, 1874, authorized an inquiry into the causes of the disease as it had occurred the previous year. For this purpose the Supervising Surgeon of the Marine Hospital Service, and a medical officer of

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<sup>1</sup> *The Cholera Epidemic of 1873 in the United States.* Washington, 1875. 8vo.

the army, were appointed to ascertain the facts, and to "make a detailed report of the information collected, on or before the first day of January, eighteen hundred and seventy-five, to the President, to be submitted to Congress."

To carry out this intention Dr. McClellan was associated with Dr. Woodworth, the Supervising Surgeon of the Merchant Marine Hospital Service of the United States. Dr. McClellan received instructions on 7th May to proceed to Louisville, Kentucky, and from that point to visit all the towns at which cholera had prevailed in 1873, but, inasmuch as there had been 264 localities infected, which were scattered over 18 States, while there were only 238 days available for the purpose, a personal visit to each was impossible, and it became necessary to issue circulars soliciting information from the various medical practitioners in these localities, who had had an opportunity of observing or collecting facts regarding the epidemic. When the replies were deficient in details further information was asked for. Dr. Woodworth addressed two hundred and sixty copies of his circular to medical practitioners in the infected districts, and received answers from eighty-four only; and, though Dr. McClellan does not mention the number he sent out, it appears from his account he did not succeed in eliciting contributions from many of whose assistance he would have been glad to avail himself, and those he did receive were often too meagre to be of much service. Drs. Woodworth and McClellan drew up separate reports, which were submitted through their respective Departments, by the date named in the original resolution.

The volume under consideration commences with the report of Dr. Woodworth, which, with its appendices and index, submitted through the Treasury Department, extends to 28 pages. The remainder is occupied by Dr. McClellan's Report, submitted through the War Department, which extends to 1025 pages, and embraces—1st, The History of the Cholera of 1873 in the United States, compiled by Dr. McClellan; 2nd, An Account of the travels of Asiatic Cholera in Asia and Europe, by Dr. John C. Peters, of New York, and in North America, by Dr. McClellan; 3rd, A Bibliography of Cholera, drawn up by Dr. John S. Billings, Assistant Surgeon United States Army, filling 315 pages. There is an outline map showing the distribution of the epidemic in 1873, and twelve plans of towns where it prevailed, attached to Dr. McClellan's history; and fourteen outline maps to illustrate the supposed routes of cholera from India to other parts of the world, in its various epidemic diffusions.

The first case of choleraic disease in New Orleans, which attracted attention, died in the Charity Hospital on 4th March,

1873, having been attacked on the 2nd. Though returned as cholera morbus in the first instance, the symptoms were so characteristic of malignant cholera that it led to inquiries as to previous cases, when it was ascertained that deaths had occurred from a similar disease on the 9th, 10th, and 28th of February, and on 1st, 2nd, and 3rd March. Dr. McClellan states the deaths from the disease were, in March 16, April 90, May 125, June 18, July 4, after which there were but single deaths in August, September, and November.<sup>1</sup> In April the disease appeared at several points along the Mississippi, and became more frequent in May and June. The epidemic was at its height in July and August at various points along the Upper Mississippi, the Missouri, and Ohio, and soon after sensibly declined in force. The disease seems to have been common in the states of Louisiana, Mississippi, and Arkansas, along the banks of the river, and for some distance back into the country; it was extensively diffused through Tennessee and Kentucky, to the south of the Ohio river, and in the portions of the States of Ohio and Indiana, on the north bank of that river, and for some way back. In the south-western portion of Illinois there was a good deal of the disease in the neighbourhood of the Mississippi, but as it went north it retired from the river more and more. In Missouri, west of the Mississippi, there were a considerable number of manifestations near that river, as well as on the course of the Missouri itself. Farther north there were outbreaks at Burlington, and Davenport, Iowa, both on the Upper Mississippi. Chicago was the only place on the great lakes which suffered from cholera in 1873. Wheeling, in West Virginia, may be considered the eastern point to which the epidemic reached, for although there was a slight manifestation at Pittsburg, there were only five cases, which occurred between the 1st and 8th August. To the south of Tennessee, Huntsville, and Birmingham, Alabama, were affected; one imported case was reported at Montgomery, Alabama, and one each at Atlanta, and Dalton, Georgia, but no others followed. There was an outbreak of considerable intensity at Denison, in the north-east of Texas, from August to October. In the north-west there was a limited, though severe eruption of malignant cholera, among a party of Swedish emigrants, in July, at Crow River, thirty miles from Willmar, Minnesota; and the

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<sup>1</sup> In No. V. of the new series of 'Reports of the Medical Officer of the Privy Council and Local Government Board,' Mr. Radcliffe gives (p. 195) a table of the deaths each month, under the three denominations of Cholera Spasmodica, Cholera Morbus, and Cholera Infantum. The total is 359, or 100 more than given by Dr. McClellan, but both agree in showing the greatest mortality in May, and a rapid decrease in June and July.

following month another, chiefly among Russian emigrants from Odessa and the Crimea, at Yankton, in Dakota. These will be noticed again in connection with a supposed introduction of virus from Europe, in their bedding and clothing. Lastly, four deaths from cholera occurred in August, at Kelton, a small town of 450 inhabitants, on the Pacific Railway where it crosses the Sierra Nevada in the north-west of Utah; no emigrants had arrived at this place previous to these cases occurring.

It thus appears the force of the epidemic was experienced along the Lower Mississippi, through the States of Tennessee and Kentucky, the portions of Ohio, Indiana, and Illinois, to the north of the rivers Ohio and Mississippi, with extensions into the States of Missouri, and Iowa, to the west of the Mississippi. Around this continuous manifestation of the disease there were outlying attacks of varying extent, at Pittsburg, Pennsylvania, Huntsville and Birmingham, Alabama, Denison, Texas, Yankton, Dakota, Crow River, Minnesota, and even Kelton, Utah, as are to be met with more or less around every considerable epidemic. During its prevalence, too, diarrhoea is reported from many points in the epidemic area to have been unusually frequent, and this was experienced beyond the cholera field altogether, as San Antonio, Texas, where diarrhoea was noticed as unusually prevalent as early as December, 1872, and continued up to June, 1873, while "several cases are said to have assimilated cholera," (p. 448). As to the latter, indeed, there are numerous notices from medical practitioners in the infected districts that cholera morbus, exactly resembling what they met with in other years, was more frequent during 1873, and many, even to the last, maintained that they encountered no new disease in 1873, as these cases were indistinguishable from those that they were familiar with in previous years, and that they arose at points, and under circumstances, which they believed precluded exposure to previous cases, or to fomites proceeding from them, while the resulting mortality was moderate.

In his report Dr. Woodworth expresses his desire to set forth, briefly, the facts which establish the connection of the mercantile marine with the importation of Cholera into the United States, and to suggest what may be done to prevent, or limit future outbreaks. Success in this latter, he admits, in common with the answer to any question, will depend upon the extent and accuracy of our knowledge of the factors of the problem. As a basis for his practical recommendations he states, compendiously, "What is known and accepted concerning the cause of malignant cholera—its origin, character, mode of propagation, transportation, &c.," in the form of nine propositions as follow:

"I. Malignant cholera is caused by the access of a specific organic

poison to the alimentary canal; which poison is developed spontaneously only in certain parts of India (Hindustan).

"II. This poison is contained primarily, so far as the world outside of Hindostan is concerned, in the ejections—vomit, stools, and urine—of a person already affected with the disease.

"III. To set up anew the action of the poison, a certain period of incubation with the presence of alkaline moisture is required, which period is completed within one to three days; a temperature favouring decomposition and moisture or fluid of decided alkaline reaction hastening the process, the reverse retarding.

"IV. Favourable conditions for the growth of the poison are found (1) in ordinary potable water, containing nitrogenous organic impurities, alkaline carbonates, etc.; (2) in decomposing animal and vegetable matter possessing an alkaline reaction; (3) in the alkaline contents of the intestinal portion of the alimentary canal.

"V. The period of morbid activity of the poison—which lasts, under favourable conditions, about three days for a given crop—is characterized by the presence of bacteria, which appear at the end of the period of incubation and disappear at the end of the period of morbid activity. That is to say, a cholera ejection, or material containing such, is harmless both before the appearance and after the disappearance of bacteria, but is actively poisonous during their presence.

"*Note.*—It is not meant by this that the bacteria so found are the cholera poison, since they differ in no appreciable manner from bacteria found in a variety of other fluids. Indeed, Lebert hints that the bacteria may even be the destroyers of the poison.

"VI. The morbid properties of the poison may be preserved in posse for an indefinite period in cholera-ejections dried during the period of incubation, or of infection-matter dried during the period of activity.

"VII. The dried particles of cholera poison may be carried (in clothing, bedding, etc.) to any distance; and when liberated may find their way direct to the alimentary canal through the medium of the air—by entering the mouth and nose and being swallowed with the saliva—or, less directly, through the medium of water or food in which they have lodged.

"VIII. The poison is destroyed naturally either by the process of growth or by contact with acids: (1) those contained in water or soil; (2) acid gases in the atmosphere; (3) the acid secretion of the stomach.

"IX. It may also be destroyed artificially (1) by treating the cholera-ejections, or material containing them, with acids; (2) by such acid (gaseous) treatment of contaminated atmosphere; (3) by establishing an acid diathesis of the system in one who has received the poison.

"*Note.*—Why, when perennially endemic in India, with which country intercourse is constant, the disease becomes epidemic elsewhere only at long intervals, is beside the present purpose to inquire. What conditions, meteorologic, telluric, and diathetic, are necessary to its epidemic spread, even in India, are not yet de-



terminated ; and no practical result for the object of this paper could accrue from speculation thereon."—(P. 8, 9.)

We quite agree with Dr. Woodworth that the conditions necessary for the epidemic spread of cholera, either in India or elsewhere, are not yet determined, and that their consideration, at present, might not have assisted him to draw up rules for the exclusion of the disease from, or its limitation within, the United States ; but he forgets that this is a direct admission that his knowledge of the factors which cause the migrations of the disease from one part of the world to another—to America among the rest—is most imperfect, and that, consequently, on his own showing, his conclusions can have little weight. We readily admit that an officer, placed as he is, at the head of a department to watch over the public health, must frequently act on imperfect information in devising the measures to protect it from injury, and may thereby be led to make recommendations which are subsequently found to be useless, if not sometimes actually hurtful. We have no desire to criticise such endeavours harshly, but, in submitting these remarks to our readers, we consider it incumbent on us, in the true interests of medical science, to point out this anomaly, and to caution them against placing implicit confidence in theories which, on the face of them, are far from representing the whole facts, and which most likely may have to undergo great modification as our knowledge extends.

Dr. McClellan opens his report with a chapter on the clinical history of the epidemic of 1873. The second chapter is on the etiology of cholera, in which he sets forth, in seven propositions, what he considers the nature of the cause and the mode of its diffusion, and illustrates his views by experiments and opinions brought together from a variety of sources after extended research. As these propositions cover much the same ground as those of Dr. Woodworth, it is unnecessary to introduce them here. The third chapter is on the prevention of cholera ; the fourth, by Dr. Peters, on the origin and spread of the cholera which reached the United States in 1873, in which he goes back to the occurrence of the disease in Persia in 1865, and gives an outline of its manifestation in India, Persia, and Europe, up to 1873, from which he believes it to have been carried to America in the beginning of that year. The fifth, and seventeen following chapters, are devoted to the details of the individual outbreaks in the different states, in which the epidemic appeared in 1873, and the twenty-third to the narration of its occurrence among the troops of the U.S. Army, to which is appended an abstract of the state of the weather from December, 1872, to November, 1873, from the monthly weather

review of the Chief Signal Officer. It is to be regretted that the evidence which has been collected by Dr. McClellan, with so much assiduity, is in too many instances deficient in that amount and precision of detail, both as regards cholera and other forms of intestinal disease, necessary to elucidate the questions involved in this investigation.

Dr. McClellan informs us in his opening chapter that—

“The vast majority of the medical men of the United States who were engaged in combating the disease during the progress of the epidemic, both in hospitals and in private practice, are unanimous in pronouncing it to have been Asiatic cholera of foreign origin ;”—

“A second class, composed of a most respectable number of gentlemen, both numerically and intellectually, recognised the disease to have been cholera in a fatal form, but announced it as American cholera, not epidemic but endemic, having its origin in certain local and malarial influences.”

A third class reject entirely the cholera hypothesis, and pronounce the disease to have been “pernicious bilious fever of an algid type,” “congestive malarial fever,” &c., and, when pressed by facts, take refuge behind such terms as sporadic or septic cholera.

“The key-note to this discussion will be found in the obscurity which surrounds the infection of the initial cases, the consideration of which is referred to the narrative of the epidemic” (p. 1).

Notwithstanding the obscurity admitted in the last paragraph, Drs. Woodworth and McClellan both adopt the conclusion that the cholera poison was introduced at New Orleans by emigrants from Europe ; and the latter gentleman, in the course of his history, gives three instances of parties of emigrants from Europe, who, on reaching their destination in the interior, unpacked their clothing and bedding, and immediately after were attacked by cholera, in consequence, he says, of the cholera-poison imbibed by these articles in Europe having become liberated on their exposure, and so having affected those in their immediate vicinity. As the supposed introduction of the disease at New Orleans and the three cases just mentioned involve a most important point, on which hitherto there has been a want of definite information, we shall examine the evidence in each, and test how far the authors' conclusions are borne out by it.

The first case of cholera which attracted attention in New Orleans in 1873 occurred in a negro, aged 29, who was attacked on 2nd March, and died in the Charity Hospital on 4th. The symptoms were so characteristic of malignant cholera that the case was brought to the notice of the Board of Health, and it forthwith instituted inquiries as to whether there had been any

cases previously. It was then found that at least six deaths had taken place before this one. Whether there had been other attacks which did not prove fatal does not seem to have been ascertained.

As the Board of Health was most anxious to obtain the fullest information on every point connected with the origin of the disease in these cases, the investigation embraced "all modes of infection and importation, as by visiting ships, the washing of clothing for persons connected with shipping, visiting of sailors' boarding-houses, contamination of drinking-water, &c. &c.," and subsequently, during the course of the epidemic, on every case being reported, it was visited and these points inquired into.

The earliest attacks of cholera in 1873 were found to have occurred on the 8th and 9th February, in two men, the first of whom had come from Pensacola two months previously, and had since been in New Orleans; the other had been in the place four years. These men lived at a considerable distance from, and had not communicated with, each other. There was then a cessation in the attacks until 27th February. From this time the attacks (or deaths) were:

On February	27, 1 male, aged 52, died.	On March	10, 1 male, aged 60, died
"	28, 1 female " 21 "	"	15, 1 " " 1 "
March	2, 1 " " 4½ "	"	23, 1 female " 2 "
"	1 male " 29 "	"	27, 1 " " 7 "
"	3, 1 " " 50 "	"	30, 1 male " 40 "
"	1 " " 18 "	"	1 " " 23 "
"	7, 1 " " 45 "	"	1 " " 27 "
"	8, 1 female " 34 "	"	31, 1 female " 3½ "
"	1 " " 2½ "	"	1 male " 45 "

Of these persons two of those marked "attacked on 30th" arrived that day at New Orleans in a river steamer, the Sabine, from Ouachita (or Washita) river, which falls into the Mississippi, a little to the north; no history of the first could be obtained; the other was attacked on 30th, after reaching New Orleans, and died on 31st. One of the men attacked on 3rd and one of those on 30th had recently arrived in the city from the country, and had no residence; the remainder were all persons resident in New Orleans, though at points much separated from each other. Four who were attacked (or died) on 27th February, 2nd, 7th, and 10th March, were labourers on the steamboat levée, where the river steamers lie; the remainder were all unconnected with shipping,<sup>1</sup> or with each other, or with previous cases, except that of 23rd March, in

<sup>1</sup> The husband of the woman attacked on 28th February, died on 27th, it is said of gastro-enteritis; the uncle of the child that died on 2nd March died the previous day of diarrhoea. Both men were labourers on the steamboat levée.

which an uncle of the child's died in the same house, but it is not clear of what complaint, though from the description it is very likely to have been cholera. This man had just come from St. Charles' parish, on the river, a few miles above New Orleans.

According to the annual report of the Board of Health to the General Assembly of Louisiana for 1873 great difference of opinion existed among the medical men of New Orleans as to the nature of this outbreak; those connected with hospitals, seeing few save fatal cases, unhesitatingly pronounced the disease Asiatic cholera, but a large portion of the remainder considered it not to have been that disease, though agreeing with the others that no distinction could be drawn between the fatal cases of the cholera sporadica they met with in 1873 and genuine Asiatic cholera. Most, however, although admitting the difficulty of differential diagnosis in fatal cases, thought the following considerations warranted the belief it was not Asiatic cholera:

"1st. The non-importation of the disease.

"2nd. That no evidence of portability or infection was exhibited in the city, where every facility existed for ascertaining its natural history.

"3rd. The appearance of the disease at many localities remote from each other, and from anything that could be considered a common cause, and in all parts of the city, at dates synchronous or nearly so.

"4th. The small mortality of those attacked with vomiting and purging, during the presence of the disease; attacks similar to which, during the presence of former epidemics, have customarily proved the beginning of that usually fatal disorder.

"The death-rate of persons thus attacked is variously estimated at from 3 to 12 per cent.

"5th. The fact that so general a diffusion of the disease existed without resulting in a great epidemic.

"Had the disease possessed the portability and infectiveness attributed to Asiatic cholera, its course and results would have been the reverse of the actual occurrence.

"6th. The fact of the long-continued presence of the disease in New Orleans, without flaming up to epidemic proportions, shows absence of the peculiar characteristics of Asiatic cholera, as generally admitted. The disorder seemed endemic, not epidemic" (p. 101).

The course of the outbreak in the valley of the Mississippi in 1873, we think, excludes every doubt as to its having been what has hitherto been designated Asiatic, epidemic, or invasive cholera, and the manifestation at New Orleans must be taken as part of the general epidemic. We do not hereby question

the accuracy of the statements of the New Orleans observers ; on the contrary, we believe they had the good fortune to witness a certain phase of the epidemic, at its outer margin, where its intensity was small, and where peculiarities could be easily and clearly distinguished, which nearer its centre would have been obscured and overborne by the greater violence of the disease, and in spite of their previous belief they have been carried irresistibly to the conclusion that, as they saw it, certain properties generally said to characterise Asiatic cholera were wanting ; in other words, we have got into the habit of ascribing to Asiatic (or, as we prefer to call it, *malignant*) cholera properties which, as it came before them, it did not present.

From the first of the reasons assigned above it is obvious the Board of Health had been unable to trace importation ; but Dr. McClellan gives extracts from a communication from Surgeon van Buren Hubbard, U.S.A., in which that gentleman states the results of certain inquiries he was requested to make at New Orleans on this subject in the course of 1874, which induced him to adopt the opinion that "the doctrine of non-importation will not stand." Dr. Hubbard admits—

"It has been found utterly impossible to establish the arrival of individuals who were personally affected with cholera, but the investigations have developed certain most significant facts" (p. 104).

These facts are shortly, first, that from December, 1872, to June, 1873 a considerable number of vessels, with emigrants, arrived from Liverpool, Hamburg, and Bremen ; second, that the examination at the quarantine station on the Mississippi, was not such as Dr. Hubbard thought was required to prevent the introduction of cholera ; third, that such of the emigrants as proceeded up the Mississippi usually remained some days in New Orleans, and that, during January, February, and March, 1873, many of these boarded at houses on the steam-boat levée, where the river steamers lie ; and, lastly, that several of the early fatal cases of cholera were in persons who frequented this levée as labourers, either on board, or in connection with these steamers, where, Dr. Hubbard adds, "the report of the Board of Health states the earlier cases of the disease became infected," (p. 110). Dr. Hubbard seems to have fallen into an error here, as the Board of Health, so far as we have seen, makes no such admission ; while very properly indicating where these men worked, the Board does not venture to specify whether they were infected there, or at their houses, or elsewhere. As to the value of these facts Dr. Hubbard remarks :

"The evidence, although circumstantial, is certainly strong that the unfortunate individuals who contracted cholera upon or near the steam-boat levee came in contact with the poison which had been imported in the effects of emigrants from the cholera infected districts of Europe, from which infected districts it is shown that emigrants did arrive prior to the occurrence of the initial case in the city of New Orleans. This is certainly more reasonable, more in accordance with history, than it is to suppose that a *de novo* development of Asiatic or epidemic cholera occurred in the beautiful Crescent City."—(P. 111.)

As reviewers we have to insist on sufficient evidence being produced in support of any opinion submitted for our acceptance, and cannot allow ourselves to be influenced by Dr. Hubbard's reference to history, or his plaintive appeal in favour of the Crescent City. We have no alternative but to state that he has not advanced any proof of the importation of cholera-poison in the effects of emigrants to New Orleans in 1873, but, in its place, has merely urged certain deductions from a current theory as to the cause of cholera (which theory, itself, has yet to be established by adequate evidence), and the deductions are directly opposed to the facts observed by the physicians in New Orleans. Whether, had he been earlier in the field and been able to investigate the subject when the disease first appeared, he should have been more successful, people may differ; but, as the matter stands, we hold he has completely failed to render probable, far less to afford anything like trustworthy evidence of the view he upholds.

The first of the instances of supposed importation of cholera-poison we shall deal with, occurred among a party of emigrants from Holland, consisting of a man named Have, his wife, and five children, the wife's sister, and two young men acquainted with the family, who left Tubbergen in that country on the 31st May, 1873. They embarked at Rotterdam for England, and left Liverpool in the City of Limerick steamer for New York, where they arrived on 5th July, no cholera having appeared during the voyage. On the 6th the party left New York for their destination, Carthage, a village about ten miles north of Cincinnati. They proceeded by the Baltimore and Ohio railway. On 9th or 10th July they reached Cincinnati, where cholera had then been prevailing about a month, and stopped during the night, "sleeping at a station house." Next morning the party proceeded to Carthage, and were received by a brother of Have, who had resided in the village for some time. On 13th July the packages containing their bedding and clothing having arrived, they occupied a house, and these, which it is said had not been undone since they left Holland,

were opened, and the various articles hung up in and around the house to dry. On 15th, a child, aged three years, was attacked with cholera; the father, who by this time had obtained employment at some gravel pits in the vicinity, was recalled from his work, and was directed by the medical attendant to cut down and remove some rank weeds growing round the house; whether this task was commenced is not mentioned, but the same day he, and a second child aged five, were also attacked. On 16th the mother was seized, on 17th a child aged seven, on 19th another aged eight, on 21st a fifth child aged four, and on 23rd the wife's sister was attacked, and died the same day. Seven in all died. The two young men suffered from diarrhoea only. On 23rd July a young married female, who lived in a comfortable house about 100 yards in rear of that occupied by the Have family, became affected with diarrhoea, which on 25th passed into cholera, and she died. It was subsequently ascertained she had prepared articles of food for the Have family, which she had, each day, carried so far only as the fence at the back of their house.

The Longview Lunatic Asylum is situated at Carthage; this is divided into a white, and a coloured portion, which are separate, and detached from each other. On the 21st July the cook of the coloured division, a female, was attacked with cholera, and died on 23rd; it is not stated whether she had been away from the asylum for some time, or near the Have family, but the supervisor of the attendants in the coloured division, a negro named Marshall, visited them frequently from curiosity, and from their house returned to the asylum, and passed a portion of each day in the kitchen there, in company of the cook already mentioned, but whether immediately after his return from Have's house, or after a variable interval, there is no information. On 23rd, after the last death in the Have's family, Marshall took a braided coat, which had belonged to Have, from the house to the asylum, and, as it was damp, hung it up on the back porch to dry; here it attracted the attention of a patient named Howard Preston, who put it on, wore it for the day and slept in it the following night, after which it was again taken possession of by Marshall. The next day, 26th July, Preston and four other inmates of the coloured portion of the asylum were taken with cholera, on 27th two others were attacked, on 28th one, and on 29th four; in all thirteen, of whom nine were females, and four males; of these six females, and three males died. Dr. McClellan attributes the introduction of the disease among the patients to virus carried in the coat, and the cook's attack, inferentially, to the poison of the disease carried by Marshall (pp. 356-358).

When we examine the various events mentioned in this summary, and test the inferences from them, we encounter these difficulties. In May, 1873, when the Have family started for America there was no cholera in Holland; it had just then commenced its migration from Poland and Bohemia, where it had been during the winter, but it was not until the middle of July that it reached Frankfort, Magdeburg, and Berlin; and at Hamburg it commenced on 21st June only, and for the first three weeks there were but six fatal cases. Dr. McClellan's theory therefore breaks down at the commencement, there was no cholera at the point of departure of the Have family to afford that supply of fomites, to which he attributes such an important influence in the development of its subsequent misfortunes. Again, the family had passed a night in Cincinnati, where cholera had then been prevailing about a month; no effort seems to have been made to trace whether they had been exposed to the causes of the disease there, the possibility of which must have been excluded before the operation of fomites could have been admitted after their arrival at Carthage. Neither have we any notice of the condition of the house the family occupied at Carthage, nor of its surroundings, though these seem to have been such as to excite the suspicions of the medical attendant. Neither does the story of the braided coat assist us in clearing up the attack at the Longview Asylum, the possibility of its having imbibed cholera discharges in Holland has already been disposed of, does the evidence support the belief that this had taken place during the sickness in the family at Carthage? We have seen the female cook at the asylum was attacked on 21st July, two days before that coat was brought to it, and, that not only the person who wore and slept in it, but four others were attacked on 26th. Now, as there were only four men attacked in all, one at least of the attacks on 26th (if not more) must have been in a woman, which shows that whatever was the cause of the disease, the women were as much exposed to it as the men, though, in the absence of direct evidence to the contrary, we cannot suppose that in any well regulated asylum the male and female patients had free intercourse with each other, and, consequently, that the women could not have been exposed to emanations from the coat. Thus, at whatever point this case is examined the evidence of propagation of cholera by fomites breaks down.

A second instance of supposed importation of cholera poison from Europe occurred in a Swedish family, named Antonson, at a settlement of their countrymen on Crow river, Kandiyohi county, in south-western Minnesota. This party consisted of the father, mother, four children, and a young man, a family



friend, named Oleson. They came by steamer from Vük, Alfoden, some two hundred miles north of the island of Rugen, to Bergen in that island, early in May, and after remaining three weeks there embarked for America in the steamer Peter Japson, and arrived at New York on 26th June. This vessel carried 298 passengers, and the quarantine records at New York show that no sickness occurred among them during the voyage. It is not mentioned when the Peter Japson left Bergen, but, inasmuch as emigrant vessels do not proceed at so high a speed as regular packets, there can be no material error in taking the duration of the passage at twenty days, which would give the 6th June as the day of her departure. The party seem to have left New York soon after their arrival, to have proceeded through Michigan to Grand Haven, and from that to have crossed Lake Michigan to Milwaukie, from which they proceeded to St Paul, Minnesota, and thence *via* Willmar to Crow River Settlement, which they seem to have reached on 2nd July. This course would carry them altogether clear of the cholera field of 1870, and while on the journey none of them seem to have suffered in health. Before leaving Bergen the effects of the family were packed up, except such articles as they required on the voyage, and were not opened until 2nd July, when additional articles of clothing were taken out and distributed.

On 3rd July Oleson got diarrhœa, which passed into cholera on 5th or 6th, and he died on 10th. The subsequent attacks were, on 6th a boy aged 9, on 9th a girl aged 11, on 12th a girl aged 2, and the same day a man named Erickson, at whose house these people had been taken ill, was attacked. All these died without medical assistance. Dr. Frost, of Willmar (thirty miles distant), was called on 12th, but on his arrival found the last cases dead; but on 13th Mrs. Antonson, the mother, was attacked, and under his care recovered; and on 16th the fifth child, a girl aged 14 years, was attacked, but recovered. All the inmates of Erickson's house had diarrhœa, and three persons from a distance, who had visited the house during Oleson's illness, are reported to have died of cholera.

Dr. McClellan requested Dr. Frost to revisit Crow river to obtain information regarding this case, and suggested to him that the infection had taken place at Bergen. Dr. Frost directed his inquiries in that direction, and was informed by Antonson that cholera had occurred at Bergen prior to their departure, and that, since their arrival in America, they had heard of the death of friends from that disease. Dr. McClellan remarks on the whole case thus:

"The above is presented as a distinct, isolated, but positive epidemic of cholera occurring in the United States during the summer

of 1873, produced not by local causes, influenced not by individual indiscretions, but by positive importation. When the trunks or chests that had not been touched since they were packed at Bergen on the island of Rugen, a port of the Baltic Sea, at which city cholera had been present during 1872 and 1873, were opened at Willmar, Kandiyohi county, Minnesota, United States, then and there was the person of Christian Oleson infected with cholera, the material of which had been conveyed from the Baltic in the fabrics of which the articles of clothing were prepared, and from Oleson, the Antonson family, and Errick Erickson, and three other persons who are unknown, were infected" (p. 442).

There is no indication of the date when Dr. Frost's inquiries were made, but if, as seems probable, this was not until after Dr. McClellan had issued his circular requesting information from the local medical practitioners, they must have been commenced a year or more after the outbreak at Crow river, and every one, who has had any experience in investigating such occurrences after that interval, must be aware of the difficulty of getting accurate information when no note was made of them at the time, and of the great liability to error when the memory alone was trusted to, especially if leading questions were put, and the answers not confirmed by independent evidence. The statement of Antonson that cholera was at Bergen in 1873, when he was there, is an illustration of this; though the disease had been there, both in 1872 and 1873, it had ceased before the end of the former year, and had not even approached it closely in the first half of the following one; great attention was given to the spread of cholera in Europe in 1873, and it is well ascertained that in its progress down the Vistula, from Poland, it reached Neufahr, at its mouth, on 15th June only, and Dantzic on 27th; it was at Hamburg on 21st June; Stettin on the Oder, to the east of Bergen, and Lubeck to the west, were not affected till August, though there was a slight manifestation at Helsingborg, in Sweden, nearly opposite to Copenhagen, commencing in July. Had cholera been prevailing in the island of Rugen at the end of May, and commencement of June, when Antonson sailed from Bergen, it is most unlikely such an important fact could have been overlooked by the able German observers, to whom in the main we owe the accounts of its progress in 1873. We cannot avoid the conclusion, therefore, that Antonson was in error, and that Dr. McClellan has as little ground for his theory of importation in this case as in that of the Have family. As the evidence then does not support the theory of propagation by fomites, and the Antonson family do not seem to have been exposed to any other cases on their journey from New York to Crow river, they must have met

with something there sufficient to light up the disease under the general conditions prevailing at the time.

The third instance adduced by Dr. McClellan to prove the importation of the cholera poison occurred in connection with emigrants from the south of Russia. These mostly proceeded from their ports of entry to Yankton in Dakota, where they were accommodated in such vacant houses as were available until they could be located on the farms in the country that had been selected for them. A considerable number had arrived at Yankton in the course of August, bringing with them boxes and bales with their bedding, &c. On 25th August, immediately upon the arrival of a party of these people, who had come directly from New York, two cases of cholera occurred in children, who, with their parents and several other families, were lodged in an unoccupied schoolhouse.

On 26th five new cases occurred in the same room, and nine others in an adjoining one. Of these sixteen cases but two recovered. The disease continued in force till about 15th September, up to which forty-two cases had occurred among the emigrants, of which twenty-nine proved fatal. The large majority of the deaths were in persons under sixteen years of age, and but few among the adults. After the appearance of the disease among the Russians diarrhoeal diseases became unusually frequent among the residents of Yankton, and a few well-marked cases of cholera appeared.

Not only were the Russian emigrants very uncleanly in their persons, but they set all hygienic rules at defiance; but this was not confined to them, for we are told—

“Early in September a lot at the corner of Third Street and Broadway was cleared for the purpose of erecting a block of buildings. The dirt and *débris* that were removed from this site were used to fill up some inequalities on Third Street. Among other refuse removed was the contents of an old privy, which had been used by some Russians who had been quartered in an adjoining building. This privy soil was scattered over the street to secure its rapid oxidization, but in so doing the stench was unendurable” (p. 468).

It will not excite surprise that several of the residents, who lived or wrought near, or had to pass this spot frequently, were attacked with cholera.

It is estimated that at least 2500 Russian emigrants arrived at Yankton during the season; the majority were from Odessa and the Crimea; where the remainder came from is not mentioned, nor do we have specific information whether those to whom the above details refer were from Odessa and the Crimea,

or elsewhere. Dr. McClellan himself seems to have visited Yankton in November, 1874, in quest of further information from the emigrants themselves, and states—

“By some of intelligence, it was admitted that the districts of Russia from which they had departed were cholera-infected, and many instances corroborative of published reports as to the prevalence of this disease in the southern provinces of Russia were obtained. One man of much intelligence and refinement recounted interestingly the frequent arrivals of the disease at the city of Odessa, and another man from the vicinity of the city of Taganrog confirmed the cholera reports published from the Crimea during the past few years” (p. 464).

The remarks of all these persons, as well as the last, must have referred to *the past few years*, and not to 1873, to which the inquiry in the present case should be limited. The men who described frequent arrivals of cholera sick at Odessa must either have been mistaken as to the facts, or must have been thinking of a previous year, for, according to the Russian government returns, in 1873 there was no attack or death from cholera in the government of Cherson, in which Odessa is situated, and in Bessarabia, the next province to the west, there were in 1873 only fifty-three attacks, the first on 20th June, and two deaths. The governments of Taurida and Ekaterinoslav, the former embracing the Crimea and the latter Taganrog, were altogether free from cholera in 1873. Indeed, this disease was met with to a notable extent that year in Southern Russia, in the governments of Volhynia and Grodno only, where the mortality was 6 and 18 in 10,000 respectively, and in a lesser degree in those of Minsk and Mohilew, where it was 3 and 1 in 10,000; and the nearest point to Odessa and Taganrog where it occurred (except Bessarabia, already referred to) was the government of Kiew, where fifteen cases occurred, the earliest on August 1st, of which ten proved fatal. We presume Dr. McClellan could not have had an opportunity of perusing these returns, but with the information they convey before us we can attach no weight to the statements made to him regarding cholera in Southern Russia in 1873, or to the inference he draws from that evidence, viz. that the emigrants imported the cholera poison from these districts in their clothing or bedding.

The route followed by these emigrants from New York to Yankton is not specified, but an inspection of the map will show that it must, for a long distance down the valley of the Ohio and up that of the Missouri, have passed through districts in which cholera was epidemic at the time, and they might

have contracted the disease which broke out at Yankton while on their journey thither; this point, as in the case of the Have family, does not seem to have engaged the author's attention, though it is clear he could not have established his inference as to fomites being the cause of the disease at Yankton until he had excluded the possibility of its having been contracted on the way to that place.

We have examined these three instances at such length not only because they form the most salient features in Dr. McClellan's history, but also because, could they have been established satisfactorily, they would have placed beyond doubt the portability of the cause of cholera to great distances, and have confirmed much that is now in dispute. On the other hand, had they, from insufficient examination of the facts, been taken as proved, they would have exercised a most injurious influence on our future advance in the knowledge of the causes of this disease. Had Dr. McClellan's view been correct, for instance, Dr. Hubbard's supposition that the emigrants from Germany introduced the cholera poison into New Orleans would have been possible, and would even have acquired some degree of probability, devoid of proof as it is; but with the refutation of the former the latter becomes nothing better than an *à priori* speculation, and the opinion of the Board of Health that the disease was intimately connected with local causes is strongly corroborated. Taking all the circumstances together, we are inclined to believe this American epidemic of 1873 is destined to throw much light on the nature and causes of cholera, for, springing up as it did without immediate connection with a previous one, without any proof of importation, and under conditions which induced a large number of medical practitioners, spread widely over the country and acting independently of each other, to think they had to deal merely with the ordinary cholera nostras, a little more aggravated than usual, it points out a relation between cholera nostras and malignant cholera which has hitherto been persistently ignored by writers on epidemic cholera, but which we apprehend must be fully examined into as the first step in any real advance.

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### III.—Lectures on Syphilis and on some Forms of Local Disease affecting principally the Organs of Generation.<sup>1</sup>

THE recent discussion on syphilis at the Pathological Society has revealed a considerable variety of opinion and doctrine even among those best versed in the study of the disease ; it has also made manifest the paucity of our knowledge on several important questions connected with it : so that the debate may at least have the good effect of helping us towards that first essential for any real and scientific progress in our study of the subject—a clear comprehension of the limits of our knowledge, and a separation of what is, from what is not, certainly known.

Some of the differences of opinion are, however, seen on examination to be more apparent than real. For instance, it seems of but little consequence whether we say that there are two kinds of venereal poison, one of which produces the local, the other the constitutional disease ; or that the venereal matter sometimes contains, and sometimes does not contain, the germs of the infecting syphilitic poison. Surely the presence or not of the contagium which gives rise to constitutional syphilis makes a very essential difference between two secretions.

Mr. Lee's recent lectures place the different kinds of venereal inoculations in a very clear manner before us. Mr. Lee is a reverent student of Hunter's works, and he points out with evident satisfaction the clearness of Hunter's distinctions between the different actions of animal poisons.

"Morbid poisons," Hunter observed, "are many, and have different powers of contamination. Those which affect the body, either locally or constitutionally, but not in both ways, he called *simple*. Those which are capable of affecting the body both locally and constitutionally he called *compound*."

"A poison may affect only the part in contact with it, and may act either mechanically or chemically, or may affect the vital action of that part. Thus, powdered glass acts mechanically, corrosive sublimate acts chemically, and the matter of cancer acts only on the living principle of the part. Another mode in which a poison acts is upon the constitution of an individual. This constitutes it a morbid poison ; as examples may be cited jail fevers, and different forms of secondary inflammation where there has been no open wound. A third form in which a poison may act is on the nervous system ; and, as examples, Hunter cites the occasional effects of poisoned arrows, honey, mussels, nux vomica, and probably the bite of a mad dog, which produces no specific effect on the injured part.

<sup>1</sup> *Lectures on Syphilis and some forms of Local Disease affecting principally the Organs of Generation.* By HENRY LEE, Professor of Surgery at the Royal College of Surgeons of England, Surgeon to St. George's Hospital, &c.

Those poisons which are liable to affect the body both locally and constitutionally Hunter calls compound or mixed."

"This mixed action is of two kinds—first, when it produces inflammation of the parts, and at the same time affects the whole constitution, as in the venereal disease; and, second, where a local disease, as the itch, is followed by some secondary complication, such as erysipelas; or like jail fever, which acts first on the constitution, and may be followed by certain local diseases; or like the vaccine inoculation, which produces a local disease, followed by a general constitutional influence."

Having set forth these Hunterian doctrines, Mr. Lee goes on to show that Hunter believed that there was a syphilitic poison which did, and one which did not, infect a patient's constitution. We think that Mr. Lee has done well in recalling our attention to Hunter's observations, and we agree with him in thinking that "if those who followed Hunter in his investigations had also remembered what he had said, much intricate confusion, much fierce controversy, and many a laboured volume, both in French and in English, might have been spared."

The different forms of primary infection are excellently described by Mr. Lee, but the great merit of his work consists in the explanations which he gives of the way in which the characters of the different sores may be modified or concealed, and therefore mistaken.

The local suppurating sore—

"Commences as a pustule and runs a definite course. When artificially inoculated, the inoculated point becomes red within the first twenty-four hours. From the second to the third day it becomes slightly raised, and is surrounded by a red areola. Between the third and the fourth day it contains a fluid more or less turbid. From the fourth to the fifth day the pustule becomes fully formed, and from this time to the termination of the disease the secretion consists of well-formed pus. Sooner or later the cuticle covering the pustule is detached, and in some instances it may be removed at the time of the inoculation, whether artificial or natural.

"This alters the appearance of the affection, but in no wise interferes with its essential characters. As soon as suppuration commences there is a loss of substance in the part, and an ulcer forms which has peculiar characters. When not interfered with by any accidental causes, it increases equally in every direction, so as to form a more or less perfect circle. The edges of the ulcer are cleanly cut, and present a sharp outline. The appearance presented is often that of a piece of skin having been removed by a punch. The edges of the ulcer are frequently slightly undermined and everted. The surface of the ulcer is irregular, sometimes presenting granulations, at other times presenting the appearance of having been worm-eaten. Often the bottom of the ulcer is covered by an adherent greyish tough matter, which probably is a part of the natural

texture, which has undergone a kind of molecular necrosis, and is in process of being separated from the subjacent living parts."

Such is the typical form of non-infecting chancre, which, having attained these characters, remains unchanged for a time, and then gradually heals. This kind of sore, says Mr. Lee, has never in his experience been followed by secondary symptoms. But the typical characters of the sore may be altered in various ways. If in its progress it invades different kinds of tissues, its shape and appearance may vary somewhat according to the tissue wherein it is situated. For instance, if it invades the areolar tissue there will be some amount of inflammatory exudation around it, which may be increased to a considerable amount of induration by irritation of the parts by friction or caustics. This induration will produce a certain amount of resemblance to the infecting form of chancre; but as Mr. Lee points out, this inflammatory induration usually fades gradually into the surrounding tissues, instead of having the abrupt termination of the hard sore; and we would add, it has not the cartilaginous hardness which pertains to the typical Hunterian sore. Yet when the suppurating sore happens to extend across two different kinds of tissue the exudation may cease so abruptly as to make it very difficult to distinguish it from the infecting sore, especially when it is remembered that the latter, when situated on the glans penis, has usually very little hardness. The secretion of the soft sore is seen under the microscope to consist of well-formed pus, whereas that of the infecting sore consists of epithelial debris mixed with serum and a few globules of lymph. But unfortunately this means of diagnosis is but seldom available, because an infecting sore may be made to furnish pus from its surface if irritated. Mr. Lee maintains, in direct opposition to Ricord, that the soft sore is auto-inoculable, and that the secretion of the infecting sore is not auto-inoculable, excepting when it has become purulent, as the result of irritation, or in its very earliest stage, before the induration has appeared. An important point insisted upon by Mr. Lee is the modification of any inoculation by its taking place upon a syphilitic constitution, and he quotes a case (No. xxxv) of a syphilitic patient who was inoculated—

"With the pus taken from a wound left in a child after an excision of the knee-joint. The pus was apparently perfectly healthy, but, falling upon a syphilitic soil, it produced, after a time, suppuration and ulceration. The appearances, as represented in a drawing, could not, at the time it was taken, be distinguished from those resulting from an ordinary syphilitic inoculation."

The infecting form of syphilis begins, says Mr. Lee—

"With some *adhesive* form of inflammation, such as a papule, a



tubercle, or an abrasion with a thickened base. This disease, characterised by the specific adhesive inflammation, cannot be re-inoculated upon the patient so as to produce the same kind of action. It has a prolonged period of incubation. It cannot be destroyed by caustic, is very certainly followed by constitutional disease, and when it heals leaves no loss of substance. The suppurating sore, on the contrary, always commences with a pustule. It can always be inoculated upon the patient, so as to reproduce the same action. It may be completely destroyed by caustic so as to leave an ordinary sore only. It is not followed by any constitutional disease, and it leaves a depressed cicatrix."

That constitutional syphilis can be conveyed by the blood is now clearly established, and Mr. Lee further shows that the admixture of blood may also impart a wonderful morbid energy to other fluids in the human body, as is shown by the increased virulence given to a gonorrhœa, or to the secretion of a suppurating venereal sore, by the contact of menstrual fluid.

Mr. Lee's explanation of this is that "the globules of lymph or pus which grow and subdivide at a certain rate, finding themselves in the presence of blood, on which they naturally feed indirectly, have a sudden and immediate impulse given to their growth." These facts have an important bearing upon vaccino-syphilitic inoculation, and show the necessity of carefully avoiding the transference of any blood together with the lymph in vaccination.

It has also been clearly proved that syphilis may be communicated by the secretion of the secondary affections, and among these it is important to recognise the diseased secretions of the mucous membranes. Hunter showed that the inflammation set up by the application of an irritant to the animal body has for its purpose the removal of the irritating cause, and that this is the case with regard to specific morbid poisons, such as the venereal, as well as in the case of common irritants. If then the poison is applied to a surface whose nature it is to secrete, the inflammation set up leads to increased secretion and the casting off of diseased products.

"On the tongue, inside of the mouth, uvula, and tonsils, the coagulable lymph is thrown out in the form of sloughs, somewhat similar to the putrid sore throat; but in the fauces and all down the œsophagus a thickish fluid, in appearance like matter, is secreted. When irritation is applied to a surface whose cuticle is thin; and where there is a secretion naturally, as the glans penis, then it sometimes only irritates so as to produce a diseased secretion."

For this reason, as Mr. Lee shows, it is very rarely that any characteristic induration is produced by the application of the syphilitic poison to mucous membranes. "In them the newly formed matter, instead of remaining, for a time at least, part of

the living being, is at once thrown off in the shape of mucous or pus-cells." For the same reason it is rare to see an indurated sore within the vagina, or on the os uteri, or in the urethra, or upon a glans penis that has been kept covered. Now this cannot be because these are not exposed to the syphilitic poison, but because the results of its application are different to what they would have been had it been applied to the skin. In proof of this Mr. Lee quotes a case in which there was a well-marked indurated chancre on the eyelid, in which the induration abruptly terminated at the margin of the skin and did not affect the mucous membrane. The application, then, of the syphilitic poison to a mucous membrane leads, as a rule, to the production of a diseased secretion, and, "instead of lymph being secreted which remains a part of the person who produced it, globules containing their own healthy and diseased actions are thrown off, and these diseased actions may by them be reproduced in kind." This is one reason why inoculations upon the skin of diseased secretions from mucous membranes are not more frequent. But Mr. Morgan of Dublin has succeeded in producing a well-marked syphilitic sore, by the inoculation of the vaginal discharge of a syphilitic patient upon the skin of another in whom secondary symptoms were appearing.

Besides this, however, Mr. Lee has shown that among the secondary symptoms manifested by those suffering from constitutional syphilis, is a urethral discharge consisting of "a viscid greyish secretion, often resembling in appearance thin oatmeal gruel," and unaccompanied by pain in micturition. The same kind of discharge may also be the result of primary infection, and be followed by specific induration of the inguinal glands and the whole train of secondary symptoms. This secretion is capable of communicating syphilis to another person, and thus it is seen that a urethral discharge, without the presence of any chancre, may be the means of communicating syphilis. Indeed, it is very rare to see an indurated sore affecting the urethra, and when it does so it is always close to, or at the orifice. Mr. Lee says he has never seen such a sore originate further back in the urethra than a quarter of an inch from the orifice. These syphilitic urethral discharges explain several discrepancies between the observations of Hunter and others upon the production of syphilis by the inoculation of gonorrhœal matter. Thus, Ricord has shown by a great number of experiments that the matter of gonorrhœa will not produce syphilis; and, on the other hand, Hunter produced syphilis on his own person by inoculating the matter of a urethral discharge. Besides this, Ricord and others have observed cases in which syphilis was apparently communicated by a urethral dis-

charge. Ricord's explanation of this is that in the latter cases there is probably a concealed chancre in the urethra, and that in Hunter's case the symptoms may have been due to some other cause. But the recognition of a syphilitic urethral discharge does away with the need for either of these improbable explanations, and thus, as Mr. Lee says—

“Hunter was right in saying that a urethral discharge might be inoculated so as to produce constitutional syphilis. Ricord was right in saying that ordinary gonorrhoea is a non-syphilitic disease; Hunter was wrong in as far as he implied that the secretion of an ordinary gonorrhoea could be inoculated so as to produce syphilis; Ricord was wrong in supposing that no urethral discharge which did not proceed from a chancre could be so inoculated; and they were both wrong in supposing that the secretions of secondary syphilitic manifestations, including certain forms of urethral discharge, could not be inoculated so as to produce syphilis upon another person not previously infected.”

Besides the ordinary Hunterian chancre Mr. Lee describes two other kinds of primary infection, viz. a circumscribed patch on the glans penis, having a red surface, as though the epithelium had been removed without exciting any irritation in the tissues beneath; it remains for some time in this condition, secreting a little fluid containing epithelial scales and lymph-globules, but without induration; and also a second form in which there is an indurated knot or tubercle embedded in the natural structure of the part, and without any discoverable breach of surface. Mr. Lee also points out that a frequent source of confusion between the local and constitutional forms of syphilitic sore is due to the modified sore produced when a person who has already had syphilis is re-inoculated. The induration in this case comes on without the usual period of incubation (the average time of which in the uninfected constitution is, according to M. Rollet's experiments, about twenty-four days), ulceration rapidly ensues, and the sore then much resembles the soft sore. But there is this difference between them—that the soft sore suppurates from the commencement, whereas in the re-inoculation there is first some adhesive inflammation, and the resulting cicatrix is not depressed; and Mr. Lee has experimentally proved that a hard sore may by irritation be made to yield a secretion inoculable upon the same patient. So that there are three different results of the inoculation of syphilitic matter:

- “1. That which occurs in a constitution not previously syphilitic, and which consists essentially in a persistent adhesive action.
2. That which generally occurs in a syphilitic constitution when the inoculated matter has been the result of some accidental cause of

irritation. 3. That which is produced by the inoculation of pus from a local suppurating sore."

"These last two actions may exist in different degrees and produce a mixed result both in artificial and natural inoculation."

*Twofold inoculation* may result from the mixture of the secretion of a suppurating sore with that of an infecting sore, for the two actions do not exclude each other. The result of the inoculation of such a mixed secretion is that the suppurating sore will first appear and run its course, and that subsequently, either before or after the healing of the sore, the specific induration will appear, and the seat of inoculation will present the adhesive inflammation characteristic of the infecting chancre. This sequence of events depends, of course, upon the different periods of incubation which pertain to the two kinds of sore. Such cases have, doubtless, led to much confusion with regard to the diagnosis of the two kinds of sore. Mr. Lee also mentions a form of re-inoculation sometimes observed, in which there is a "red raised pimple on the mucous membrane, depressed in its centre; and persisting for many weeks. The appearance presented is not unlike a single projection of the surface of a raspberry." There is no doubt, however, that after a time the effects of a syphilitic inoculation may disappear, and that then a re-infection may take place and be followed by secondary symptoms; but as long as the patient remains syphilitic he is incapable of being re-infected with constitutional syphilis.

For many reasons it is important to be able to determine whether a patient is still syphilitic or not, and on this point Mr. Lee considers that there is nothing so much to be depended upon as the condition of the inguinal glands. In an uncomplicated case of primary infecting syphilitic inoculation, the inguinal glands take on the same adhesive form of inflammation as characterises the original sore, and they assume the condition called amygdaloid. These glands do not suppurate, but remain firm, almond shaped, enlarged, and separate, as long as the syphilitic infection continues. They are, therefore, very valuable evidence of the person in whom they are found having had syphilis, and Mr. Lee believes that "as long as they remain, the patient, when from any cause he gets out of health, will be subject to a recurrence of some syphilitic manifestation," and that "no such patient can be said to be free from syphilis or fit to marry." Of course the absorption of any other kind of matter may cause suppuration of the inguinal glands in a syphilitic person as well as in one who is not syphilitic, but the inguinal glands do not suppurate in consequence of the absorption of the infecting form of syphilitic matter. There are, no doubt, other forms of enlargement of the inguinal glands which may

be mistaken for the amygdaloid glands of syphilis; but this would only be the case from an examination at any one period, and if their origin and progress is observed, they doubtless give a very correct indication of the condition of the patient's constitution as regards syphilitic disease. According to Mr. Lee, "if the glands became enlarged shortly after the patient had contracted syphilis, and remained as separate, hard, distinct vessels, not involving the surrounding areolar tissue, then their existence would be a sign of the patient being syphilitic." Mr. Lee points out that if syphilis occurs in an otherwise healthy person, and runs its course without accidental complications, the symptoms, both in the primary and secondary stages, are characterised by the same adhesive action; and he well says that the classification of secondary and tertiary symptoms is by no means easy.

"Practically such distinctions are, in my opinion," says Mr. Lee, "of little value, and often lead medical men to treat the name which they may happen to apply in a particular case, rather than the disease itself. . . . I am satisfied that no such classification can be practically relied upon, either as a matter of pathology or with regard to treatment. A node, for instance, which is generally supposed to be among the latest manifestations, will sometimes be the first symptom to attract attention; and, on the other hand, I have seen a well-developed scaly eruption on the arm of an Indian officer who had had no primary affection for seven-and-twenty years."

And Mr. Hutchinson remarks—"On examining the question as to the relationship between the several stages of syphilis, it is, I think, a matter of necessity that we admit that many features and many tendencies are shared by the phenomena which occur in all;" and he goes on to show that we know very little of the visceral pathology of the secondary stage, and that there are no good grounds for adopting Lancereaux's definition of the tertiary stage as the "period of gummy deposits," for that gummata may be found in the secondary stage, and are very common in the corresponding stage of the inherited disease. But Mr. Hutchinson does endeavour to make a very important distinction between secondary and tertiary symptoms, inasmuch as he considers the tertiary symptoms in the light of *sequelæ*, and not as a stage of the blood disease. When the tertiary period is arrived at, Mr. Hutchinson considers that the blood disease is at an end, and that the gummata are "probably regrowths in structures left behind from the secondary stage; that the tertiary growths result, not from blood contamination in any way, but simply from local renovation of long-resting germs." Mr. Hutchinson's chief argument in favour of this view of tertiary symptoms is their non-symmetry. But here

we think that Mr. Hutchinson's desire for reducing the phenomena of syphilis into an orderly correspondence with those of the specific fevers has carried him into error, and that his arguments for placing the tertiary symptoms outside the period of blood-disease entirely fail. In the first place, as Sir William Jenner pointed out, blood-diseases do not display by any means a constant symmetry in their manifestations, *e. g.* the eruption of typhoid fever is particularly unsymmetrical; again, as Dr. Moxon showed, some of the most striking manifestations of symmetry are seen in tertiary syphilis, and he instances the familiar occurrence of symmetrical tertiary ulcers, and also cases of symmetrical cerebral gummata. Many of the secondary symptoms, again, are not at all symmetrical, and some purely local affections are so. Mr. Lee has also alluded to the fact, mentioned by Mr. Hutchinson, that gummata may be found in the liver at the same time that there is a secondary eruption upon the skin, and it is difficult to imagine the eruption to be a manifestation of the blood-disease, and the gummata to be due to something else. The most important distinction between the different manifestations of constitutional syphilis is that laid down by Mr. Lee with regard to its treatment:

"At whatever period of the disease we find the existence of the specific adhesive form of action, whether developing itself as a primary manifestation in the shape of an indurated sore, or as an affection of the inguinal glands, or in the form of papular, tubercular, or scaly eruptions on the body, mercury is, in my opinion, sure, if properly administered, to be beneficial. When the disease, whether primary, secondary, or tertiary, has a tendency to produce suppuration in the affected parts, mercury should be administered with great caution."

There is but little difference of opinion now about the value of mercury in the treatment of syphilis, but there is still considerable variety in the mode of its administration. Mr. Lee calls attention to the practice of Pearson, who had immense experience in the mercurial treatment of syphilis, and who, as well as Sir Benjamin Brodie, who followed him, preferred to all other methods, that of inunction. A still better plan is that of the calomel vapour bath, administered according to the plan introduced by Mr. Lee. In this way the curative effects of the drug are obtained without any of the evils which pertain to other methods of administration, and the dose and action can be regulated with the greatest nicety. The value of the iodide and bromide of potassium consists rather in the removal of symptoms, than in the cure of the disease, and sarsaparilla is chiefly of value in the treatment of the bone affections.

In considering the doctrines of Hunter with regard to syphilis,

it is interesting to observe how the most recent investigations into the subject have confirmed many of his most important observations, and to note also how the philosophical general principles at which he had so laboriously arrived, preserved him from the errors into which some of his successors have fallen, and enabled him to anticipate some facts which have only recently been clearly established. As Mr. Lee says, "Some of his doctrines require to be modified by the light of more recent investigations; while others, which have been long neglected, will still serve as landmarks for this and for succeeding generations." It is pleasant to see a Hunterian professor treating his subject in so truly Hunterian a method as Mr. Lee has in these lectures displayed.

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#### IV.—Bulbar Paralysis.

THE energy and activity that have been manifested within the last few years in the department of neuro-pathology have brought about many additions to the nomenclature of diseases of the nervous system, additions not always permanent, and not seldom having to give way to others possessing more definite attributes.

Until within a comparatively recent period the medulla oblongata, although the seat of the nuclei of the nerves essential to the performance of the functions of animal life, has not been the object of pathological research in any degree commensurate with the importance of its complex anatomical and physiological relations, or proportionately with the pains that have been bestowed upon other portions of the nervous centres. M. Claude Bernard was one of the earliest to direct attention to the pathology of the medulla oblongata, through his experimental researches upon the production of polyuria and glycosuria in connection with lesions of the fourth ventricle.

Trousseau in 1841<sup>1</sup> wrote a memoir founded upon a case of paralysis that came under his notice at that time. Twenty years later the same distinguished observer recognised in this case the disease which Duchenne had then traced to its true pathology and described as glosso-labio-laryngeal paralysis in the 'Archives Générales de Médecine.' In the mean time M. Duménil had, by aid of the microscope, discovered a morbid condition of

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<sup>1</sup> 'Lectures on Clinical Medicine,' by Trousseau, translated by Victor Bazire, M.D. London, 1867.

the nerve-roots in a complicated case of paralysis of the tongue and progressive muscular atrophy. M. Trousseau in several fatal cases observed atrophy of the roots of bulbar nerves and increased consistency of the medulla oblonga from increase of the connective tissue of the dura mater. We shall see that subsequent microscopical investigation has pointed out the special structures which in the medulla oblongata present the morbid appearances now usually associated with this form of disease.

M. Duchenne, to whom we owe the name "glosso-labio-laryngeal paralysis," traced further the atrophy of nerve-roots described by Trousseau, Duménil and Wilks, to atrophic degeneration of the nerve-cells of the nuclei of the hypoglossal, vagus, facial and trigeminal nerves. The close connection or grouping that exists among these nuclei has been demonstrated by Dr. Lockhart Clarke. Later researches by Charcot, Jaccoud, and others, have confirmed the observations of Dr. Duchenne. Dr. Dowse adopts the epithet "bulbar" as the more scientific, since, while glosso-labio-laryngeal certainly expresses the prominent symptoms, it leaves out of view the seat of lesion of the nervous centre.

The degeneration of the nerve-cells of the bulbar nuclei consists in alteration of their forms, and the diminution of their sizes, even to their fading into merest traces of their former characteristics. Those cells which have not advanced so far in the process of disintegration present to low powers of the microscope the appearance of black specks. These, when further resolved by higher powers, are found to consist of cells which have lost their normal translucent characters, and are more or less filled with dark opaque granules, the nucleus being lost among these, or having become broken up into the same granular material. The surrounding neuroglia and medullary substance have also suffered a change, having become converted in some instances into forms of *sclerosis*, miliary, insular or disseminated, from increase or degeneration of the connective elements—patches of Clarke's "granular degeneration."

The characters of insular sclerosis as seen in the brain are thus described by Dr. Moxon;<sup>1</sup> the description applies to the same lesion in the medulla or cord.

"It appears in the form of circumscribed patches, generally circular on section when small, but growing more irregular as they enlarge to the size of a hazel-nut or larger. They have very much the appearance of grey cerebral matter, but are tough and firm, the whole brain being also much firmer than natural. When small

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<sup>1</sup> 'Lectures on Pathological Anatomy,' Wilks and Moxon, 2nd edit., p. 230, 1875.



and circular they often appear to surround a congested vessel, but as they grow larger this is not evident; the vessels in the mare, however, always rather large and full of blood. They appear to us to arise at small points and spread excentrically like an eruption; when small they have a dark grey colour; as they enlarge they grow paler, until at last they are with difficulty distinguished from the surrounding structure, but they have a light ochrey opacity and slightly curdy appearance, different from the creamy pink tint of healthy brain. They very seldom invade the grey matter; when they do the cells of the grey matter persist longer than the white fibres. By the microscope you will find that the nervous elements are greatly wasted away, a quantity of subfibrillar hyaline material is present, in which are countless granule-masses, and often also a variable number of amyloid corpuscles."

A portion of brain or spinal cord that has undergone sclerosis is readily detected, even by the naked eye, when carmine or other dyes have been used in the preparation of thin sections. A section thus prepared will exhibit the sclerosed tracts distinctly marked out, the healthy portions presenting their translucency and clearly showing the myeline and axis cylinders of the divided nerve-fibres, while the morbid tract has, under a low power of the microscope, a generally confused and partially opaque aspect. On submitting this portion to the scrutiny of the higher powers, *e.g.* a quarter or eighth of an inch objective, the section will exhibit instead of the cut ends of nerve-fibres with tinted axis cylinder and surrounding uncoloured transparent sheath of myelin, an irregular network of connective tissue, intermingled with which are disintegrated or enlarged connective nuclei, fragments of myelin, and deposits of miliary degeneration. In the so-called "medullary rays," which are but the normal septa of areolar tissue binding together the longitudinal bundles of white fibres of the medulla, we find the traces of dilated and wasted vessels and broken-up nuclei of the neuroglia. The original radiating septa have disappeared from the sclerosed parts, which in the untinted cord present generally a lighter colour than adjacent healthy substance. In a recent, unhardened section the sclerosed substance shows frequently fat or oil-globules surrounding the remains of the wasted vessels of the cord. The essential element of sclerosis is thus seen to be hyperplasm of the connective tissue followed by atrophy of the other structures of these organs. Bulbar paralysis does not, however, always connect itself with sclerosis, although it inevitably follows the invasion, by this change, of the fourth ventricle and the nuclei of the nerves of the medulla oblongata. The forms in which this lesion presents itself in this centre are well represented by the woodcuts

accompanying M. Bournville's edition of Charcot's lectures.<sup>1</sup> It should not be overlooked that the condition most frequently arising out of insular sclerosis is that of muscular tremors as seen in paralysis agitans and allied affections. Softening and grey degeneration of the medulla, together with atrophy and pigmentation of cells of the bulbar nuclei, are also as frequently associated in bulbar paralysis.

M. Charcot,<sup>2</sup> viewing the medulla oblongata as a continuation of the spinal cord, and having observed that cases of muscular atrophy are associated with atrophy or degeneration of the anterior cells of the cord, directed his researches to the condition of the hypoglossal nuclei, in a case of atrophy of the tongue, and has subsequently traced a similarity between the condition of the cord and the medulla in several other forms of disease.

Dr. Hallopeau,<sup>3</sup> in his monograph, has collected the observations of other pathologists, adding thereto the results of his own investigations, and dividing the cases of bulbar paralysis into three groups—1, those arising from lesions of the nuclei or radial fibres; 2, lesions of the conducting fibres to the cerebral or spinal centres; 3, those arising from lesions of cerebral fibres connecting the cerebral ganglia with bulbar nuclei.

This division, the author urges, has not only a physiological interest, but has also a clinical value, inasmuch as these several groups present differences which should be recognised. Thus, in the first group the paralysis is on the same side as the lesion. In the second the reflex as well as the voluntary movements are affected. In the third group the paralysis is habitually associated with muscular atrophy. In the first and third groups the paralysis is limited to parts supplied by bulbar nerves; in the second the affection implicates also parts supplied by spinal nerves. Pathological facts, referable to this second division, show that the motor influence is not wholly transmitted by the decussating fibres of the pyramids. Dr. Brown-Séquard has drawn attention to this pathological fact by the record of cases of paralysis on the side of lesion in the brain.<sup>4</sup> M. Vulpian relates a case of complete atrophy of one of the anterior pyramids in which no paralysis existed, and another in which the lower extremities only were affected. M. Hallopeau also gives a case (No. xxix) of lateral lesion of the medulla oblongata, attended by paralysis of the limbs on the same side, showing that a portion at least of the fibres do not

<sup>1</sup> 'Leçons sur les Maladies du Système Nerveux,' 3me fascicule. Paris, 1873.

<sup>2</sup> 'Archives de Physiologie,' 1869.

<sup>3</sup> 'Des Paralysies Bulbaires,' Paris, 1875.

<sup>4</sup> 'Lancet,' Jan. 1, 1876.

pass by the decussation of the pyramids. M. Vulpian divided the medulla in the middle line from before backwards, through the decussation of the pyramids, and yet the animal could perform voluntary movements, showing also with the above pathological observations that the decussation, at all events of all fibres, is not complete.

M. Hallopeau divides the lesion of sensation also into three groups, answering to the preceding division of motor affections.

The functions that are directly influenced in pathological conditions of the bulb are expression, phonation, mastication, deglutition. Bearing in mind that the medulla oblongata is at once a centre of innervation and an organ of transmission, it is obvious that any lesion or pathological condition must involve some of the most important vital organs in the hazards of paralysis. Flourens, long since, arrested respiratory movements by incisions into the calamus scriptorius in the floor of the fourth ventricle (*le nœud vital*). So also in forms of bulbar paralysis death often results from interruption of the respiratory functions. M. Duchenne has observed, in cases of glosso-labio-pharyngeal paralysis, that the heart's action has become greatly accelerated, irregular, and at last exhausted.

The medulla oblongata, moreover, is regarded by some physiologists as a centre of vaso-motor influence, although this view is contested by no less an authority than M. Vulpian. It is nevertheless true, M. Hallopeau states, that a section in the upper part of the bulb produces a considerable reduction in vascular tension, and dilatation of the vessels deriving nervous influence from thence. Cl. Bernard observes that a puncture in the middle line of the floor of the fourth ventricle, in the space comprised between the origin of the auditory and pneumogastric nerves, produces an augmentation and alteration of the renal and hepatic secretions; and, adds M. Hallopeau, pathological lesions of the bulb are sometimes attended by polyuria and diabetes. The salivary secretion in many instances of bulbar paralysis is notably increased.<sup>1</sup>

Dr. Hughlings Jackson some years since recorded a case of paralysis of the right side of the tongue with wasting, paralysis of the same side of the palate and of the right vocal cord, and slight weakness of the limbs, especially of the right side. This case, as described by Dr. Jackson, would now be classed as one of bulbar or glosso-laryngeal paralysis. It is, moreover, interesting from a note that accompanies it from Dr. Lockhart Clarke, and which contains a comparative illustration of pathology:—

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<sup>1</sup> See also Dittmar, quoted in 'Medical Record,' vol. ii, p. 133.

<sup>2</sup> 'London Hosp. Rep.,' 1864, p. 367.

"In fishes the pneumogastric has two separate sets of roots, a *lower non-ganglionic* and an *upper ganglionic* set. The lower set supply the nerves that go to the tongue, the first brachial arch, &c., and have themselves a double origin within the medulla oblongata. Now, in fishes there is no separate hypoglossal nerve; but Dr. Clarke has found that there is a hypoglossal *nucleus*, a mass of grey matter which occupies exactly the same position in front and at the side of the canal, and contains exactly the same kind of cells as the hypoglossal nucleus of higher animals; but the fibres which arise from it, instead of running forwards and outwards as a separate hypoglossal nerve, pursues a lateral course as the anterior root of the vagus, supplying branches to the tongue."

M. Hallopeau, as well as Dr. Dowse, proposes to dismiss the use of the term glosso-labio-pharyngeal paralysis, proposed by Duchenne, and to substitute the expression "bulbar paralysis" to indicate a paralytic affection which, in Duchenne's words, attacks successively the tongue, velum palati, and orbicularis of the lips, producing in consequence progressive difficulty of articulation and deglutition, complicated at a later period with dyspnoea, syncope, and lastly starvation. The paralysis of the tongue induces such difficulty in speaking and swallowing that it becomes impossible for the patient to retain even morsels of food in his mouth without the application of his hands to prevent its falling out. Suffocation is not unfrequently threatened by the access of food or saliva to the larynx through default of the normal directing power of the tongue. The articulation of certain letters is interfered with, so that speech becomes at last indistinguishable. The reflex sensibility also of the mucous membrane of the larynx, trachea, pharynx, and œsophagus, as pointed out by M. Krishaber, becomes so impaired that touching any of these surfaces, although it may be felt, induces no reflex muscular action.

The course of this affection is usually to a fatal termination; "it constantly progresses, it never retrogrades," observes M. Hallopeau. We shall subsequently cite from Dr. Dowse an exception to this rule. Death is often rapid or sudden, occurring through the lungs or heart. With reference to this point, M. Duchenne considered that a certainly fatal sign was to be found in the loss of the power to close the jaws. He had seen this symptom precede the fatal termination by some time. There is here a paralysis of the pterygoid muscles, showing an extension of disease to the motor nuclei of the fifth pair of nerves.

Bulbar paralysis is frequently complicated by phenomena which indicate an extension of disease to more distant portions of the nervous centres, and presents also one form in which

atrophy of muscle is a prominent, almost distinctive feature. When this atrophy is present it is usually observed in the upper extremities, attacking first the higher parts; thus, the muscles of the neck and shoulder become affected before those of the forearms and hands. In some cases the peripheric muscular atrophy precedes, in others follows, the bulbar paralysis. This occurrence M. Duchenne regarded as a simple coincidence, not associated with any invariable pathological condition. It may, however, here be noted that the order of the appearance of the phenomena, as above stated, is the reverse of that observed by Friedreich of Heidelberg, who shows that in the majority of cases of muscular atrophy the order of the phenomena followed is from the periphery to the centre.

M. Hallopeau observes that we have had no opportunity of determining this point, since no autopsy is on record in a case purely of Duchenne's glosso-labio-pharyngeal paralysis. The histological examinations hitherto published, *e. g.* those of Charcot, Joffroy, and Duchenne, have all been from cases complicated by peripheral muscular atrophy. It is only recently, moreover, that pathologists have possessed efficient means of investigating the minute changes of the nervous centres under disease. To M. Charcot is due the honour of having first traced these lesions in the form of paralysis now under consideration. He has found them to be connected with degeneration of the cells in the bulbar nuclei, more particularly in those of the hypoglossal nucleus. This condition of the nerve-cells in the spinal cord is the dominant lesion in these cases, as it is also in progressive muscular atrophy. The readers of the '*Archives de Physiologie*' will be familiar with these, as with other valuable contributions of MM. Charcot and Joffroy to nerve pathology.

We follow M. Hallopeau in tracing the symptoms to their dependence upon ascertained lesions. Starting with the hypoglossal nucleus, we have the embarrassment of speech with which the history opens; then, proceeding to lesions of the spinal and facial nuclei, we account for the affection of the voice and atrophy of the lips; the lesion at the origin of the motor nerve of the inferior maxilla announces paralysis and atrophy of the pterygoids; ascending higher, we meet with morbid conditions of the nuclei of the pneumogastric, to which are due fatal dyspnoea and syncope. It is, then, intelligible how the symptoms become limited to the nerves emanating from the lower portion of the medulla, and fail in those parts that derive their innervation from above the pneumogastric.

Dr. Hallopeau, after discussing the point at some length, concludes that neither etiology nor symptomatology furnishes

any ground for a broad distinction between muscular atrophy and glosso-labio-laryngeal paralysis. The two are closely allied, the one predominating in one case, the other in another instance—the difference, he adds, is more apparent than real. The principal features of these affections are cited in confirmation of the views of the author.

They frequently coincide; loss of motor power often coincides with maintenance of electro-muscular contractility; in both cases the lesions of the nervous centres, of the nerves, and of the muscles are identical—consisting essentially in atrophy of the primitive motor nuclei. In short—both lesions have the same anatomical seat, and follow a similar course, provoking analogous functional disturbances. The lesions of the nerve cells, according as they differ in their localisation, will give a modified character to the malady. There will thus be a bulbar form, a spinal form, and a mixed or bulbo-spinal form, corresponding respectively to the glosso-labio-laryngeal, the progressive muscular, and the complicated forms of Duchenne. To Dr. Dowse we owe the latest contribution on the pathology of this affection, and not only with reference to its pathology but to its therapeutics also, since he has placed on record a triumphant instance of recovery by early treatment through means of artificial feeding.<sup>1</sup> The restored patient has been shown at a meeting of the Clinical Society of London. Dr. Dowse agrees with some other authors in expressing his opinion of the rarity of the occurrence of progressive bulbar paralysis without lesion of spinal nerves, especially of those derived from the cervical portion of the cord, or the pons, or crura.

This affection Dr. Dowse considers to be of an inflammatory nature, and he likewise holds that it may be of a secondary or pseudo-nature from lesions, such as hæmorrhage or embolism in the pons, producing absolute inhibition of the medullary nerve-centres. In these cases no changes may be found in the nerve cells of the bulbar nuclei. The pathological conditions giving rise to this form of paralysis Dr. Dowse divides into direct and indirect.

The *direct* are—1. Progressive interstitial neuritis. 2. Thrombosis of medullary vessels. 3. Hæmorrhage. 4. Morbid growths. 5. Vascular spasm.

The *indirect*—1. Reflex action from peripheral irritation. 2. Inhibition from shock to central cerebral ganglia. 3. Intrinsic co-ordination.

Professor Kussmaul, of Freiburg, has drawn a very complete

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<sup>1</sup> 'British Medical Journal,' 1876.

picture of bulbar or "glosso-labial paralysis," in which the features closely resemble those recorded by other observers, both with reference to the symptoms and the pathology of the disease; the latter being almost unanimously explained by the histological changes discovered in the motor nuclei in the floor of the fourth ventricle. That such lesions are always found when the medulla oblongata is carefully examined by aid of microscopical investigation has been held as evidence that herein is to be found the primary seat of disease. From this view, however, Professor Friedreich, of Heidelberg, dissents. The opinions of this distinguished pathologist, with reference to muscular atrophy and pseudo-muscular hypertrophy, have been laid before our readers in a previous number of this Journal. It may here suffice to recall the opinion of Friedreich, to the effect that the lesions of the nervous centres in muscular atrophy are not primary, but secondary upon trophic changes induced in the nerves by pre-existing myositis, acute or chronic. The same views are maintained by that author with reference to the pathology of bulbar paralysis.<sup>3</sup> In his general account of the characters of this affection he dwells also with some emphasis upon its complication with muscular atrophy of the upper extremities, with which, indeed, he would seem to regard it as always combined. The lesions traceable in the medulla oblongata he regards as secondary, and attributes to an extension of an ascending neuritis from the cord below. At the same time Friedreich states that he has seen muscular atrophy of the muscles of the face, cheeks, and tongue, existing entirely independently of bulbar paralysis. Whether ultimately the evidence in favour of this last-named form of paralysis may hold its ground is open to doubt. Professor Kussmaul observes—

"We are unfortunately unable at present to give a satisfactory physiological explanation on which to found a theory of bulbar paralysis and progressive muscular atrophy, for in many cases the amount of paralysis and of atrophy of muscles are not proportional to one another. On the other hand there are numerous anatomical proofs that the muscles are trophically dependent on the ganglion-cells of the medulla, yet the disappearance of the ganglion-cells does not stand in direct relation in all cases to the atrophy of muscles."

Duchenne and Joffroy have inferred from pathological

<sup>1</sup> "Progressive Bulbar Paralysis and its Relationship to Progressive Muscular Atrophy," Clinical Lectures by various German authors. New Sydenham Society, 1876.

<sup>3</sup> 'Ueber Progressive Muskelatrophie,' &c., von Dr. N. Friedreich, Berlin, 1873, cap. ix.

research that the ganglion-cells in the anterior cornua of the spinal cord and the motor nuclei of the medulla are really of two kinds—motor and trophic.

The progressive muscular atrophy may be due to lesion of trophic cells secondarily involved, as Friedreich suggests, by preceding trophic changes in the peripheric branches and in the trunks and roots of the nerves. Where the paralysis is unattended with muscular atrophy, a circumstance of some rarity, we may suppose the lesion to be in the motor cells only, and to have originated there. In this way opposing views may be reconciled.

Professor Kussmaul, from his own observations and those recorded by others, states that this form of paralysis is seldom met with under thirty years of age, that the frequency of its occurrence increases with the number of years. Sex seems to have some influence in its etiology, inasmuch as it is far more frequently met with in males than in females. Heredity cannot be traced, nor can social position be said to exert any control, since it is met with in all grades of society. It is, however, occasionally associated with syphilis, and when this is the case it offers the best prospect of recovery, as in a case published by Dr. Silver. It is usually slow in its progress and insidious in its onset; Dr. Wilks has, however, recorded an instance where its onset appeared to be sudden. Dr. Dowse also records four cases associated with peripheral paralysis, in which the attack was sudden.

With reference to the relationship subsisting between bulbar paralysis and progressive muscular atrophy, Kussmaul points out that the two are often intimately associated, either at the commencement or at the close. The spinal cord becoming implicated, exhibits the same changes in the motor columns as are found in the medulla oblongata. As to the condition of the muscles, there is, he adds, no essential dissimilarity between progressive muscular atrophy and progressive bulbar paralysis. Atrophied and simply paralysed muscles, alone or in groups, may be found side by side. Hayem, for example, reported the case of a man suffering from progressive muscular atrophy, in whom paralysis of the diaphragm was shown to exist. Later on, Duchenne himself found in the same patient the serratus magnus, sterno-cleido-mastoid, and the scaleni paralysed. The autopsy found all these muscles attenuated in correspondence with the general emaciation, but in other respects normal.<sup>1</sup> The resemblance is also striking between these two affections, as regards their steady progress usually to a fatal termination, the shorter duration of bulbar paralysis being due, doubtless, to the physiological

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<sup>1</sup> P. 35.



importance of the parts in which it is seated. The myopathic origin of either, Kussmaul attributes to misconception arising out of macroscopic observation, whereby the coarser changes have been dwelt upon to the exclusion of the minuter or microscopic lesions. This author goes on to observe—

“When a man like Lockhart Clarke, so remarkably conversant with the histological and pathological anatomy of the spinal cord asserts that in not less than eight cases of muscular atrophy, several of which presented the particular type of the progressive disease, he found the most unequivocal lesions of the grey substance of the spinal cord, and its nerve-roots in the form of a degenerative atrophy. But not only in England, also in France and Germany, with every year there is an increase in the number of minute investigations, which place beyond a doubt the fact of a causal connection between wasting of muscle and atrophy of the grey substance of the spinal cord, particularly of the large cells of the anterior cornua. . . . Charcot has good reason for assigning almost dogmatical importance to the fact, that in these atrophies the region of the altered portion of the grey matter of the spinal marrow corresponds with the points of exit of the motor nerve-roots which supply the atrophied muscles with nerve-twigs; W. Müller also states that the number of muscles in a state of degeneration keeps pace in an approximate degree with the atrophy of the anterior ganglionic cells” (p. 36).

As an additional argument for the central origin, or against the myopathic origin of progressive muscular atrophy, Kussmaul adds:—

“In conclusion, the investigations of Vulpian and Dickinson show that the loss of large masses of muscles, by amputation of entire limbs, never induces a degenerative atrophy of the ganglion-cells, but only, very slowly, a simple atrophy, moderate in amount, of the corresponding portions of the spinal cord; at all events, no adequate diminution of the ganglion-cells attends the loss of the muscles.”

To the inference conveyed in the preceding paragraph, and especially to its last clause, we venture to demur, declining to accept it as affording the proof it is supposed to convey. We have beside us at this moment a well-prepared transparent section from the lumbar region of the spinal cord of a subject upon whom amputation of one of the lower limbs had been performed several years previously. In this example the large anterior cells of the amputated side differ so obviously from those of the other, that from these appearances alone it was possible to say on which side the amputation had been performed. The nerve-cells of the anterior horn on this side of the cord have lost their distinctness of outline; they are many of them shrunken, and, instead of presenting clearness, exhibit the

semi-opacity of granular or pigmentary degeneration; they have not taken the carmine dye, as seen in the normal cells of the opposite side, and in many their processes have disappeared.

Kussmaul, moreover, urges that the changes which, up to the present time, have been demonstrated in the spinal cord from progressive muscular atrophy, are manifold, "and have nothing specific about them—nothing which belongs only to this disease." He enumerates the various forms of softening and degeneration that have been described by Clarke, Charcot, and other pathologists, and shows that these have been equally found in tetanus, chorea, and other affections. This is doubtless true, as also that they may be regarded as different forms or phases of myelitis. The connection or association, however, is not so close between these pathological discoveries after death, and the clinical phenomena during life; and Kussmaul admits, in a subsequent sentence, that "to clear up the obscurity there is need of still further exact clinical anatomical investigations, and still more so of physiological experiments" (p. 41).

"The perfectly typical classical cases of progressive muscular atrophy have, indeed, this character of the pure, nutritional myopathia, and they almost tempt one to regard the degree and extent of the muscular degeneration as a gauge of the force and extension of the same affection in the ganglia. But this formula is open to the objection that in many cases the paralysis and muscular atrophy are not proportional to each other . . . We have, therefore, a contradiction before us; on the one hand numerous anatomical accounts are in favour of a nutritional dependence of the muscles upon those ganglion-cells; on the other, the wasting of the ganglion-cells is not in direct proportion to that of the muscles."

The doubt here expressed may be partially explicable upon the view of this affection taken by Friedreich. The degeneration and atrophy of nerve-cells may, it is suggested by Brown-Séquard, be due to irritation; this irritation may become augmented into inflammatory action invading the nutritional cells, involving their rapid wasting and extension to the motor cells; hence the paralysis of parts supplied by either the bulbar or spinal nerves. Thus it may fairly be inferred that here, as in many other difficult and disputed points, the truth may lie midway. It is quite possible that these paralytic forms of atrophy may sometimes be of a peripheral origin, and sometimes have their primary seat in the nervous centres. The question that remains to be determined is, Which is the more frequent occurrence? We doubt not that the accumulating facts of pathological anatomy direct our attention to the latter.

Another inference that may be drawn from the study of these

affections is, that their separation as distinct maladies constitutes one of those additions to our nomenclature of disease that is at present wanting in definite attributes—that they present rather the features of dependent or associated phases of the same pathological condition.

### V.—Lunacy in the United States.<sup>1</sup>

COMPARATIVE mental pathology is, as yet, an unexplored, even an untrodden field of inquiry. Comparative psychology may, ultimately, in the hands of Herbert Spencer,<sup>2</sup> be worked and welded into a shapely and serviceable scientific instrument, but few and isolated and unimportant attempts have been made to compare or contrast the deviation from healthy mental action in different races or in the same race under different circumstances and influences. Content with the observation of similarity in the origin, symptoms, and issue of morbid affections in different countries and communities, and with the adoption of the same names, medical philosophers have refrained from all efforts to determine whether the diseases thus designated are identical in nature, origin, and course, with those with which they have been accustomed to examine or to treat. The vastness of the undertaking and the paucity of the materials accessible may have justified this abstention. It has been recorded that the children of many semi-savage tribes, such as those of the inhabitants of the Sandwich Islands, display a precocity in such faculties as acquire or apply a knowledge of external objects, and, what is less extraordinary, in the muscular pursuits and pastimes of their parents, which is unknown among civilised or cultured peoples, even under the influence of training and education. This prematurity, however, never ascends into higher grades of intelligence, and the possessors continue to be incapable of all abstract notions and reasoning. Similar acuteness and aptitude have been noticed in the pro-

<sup>1</sup> 1. *Les Aliénés aux Etats-Unis, Legislation et Assistance.* Par FOVILLE, 1873.

2. *American Journal of Insanity from 1844—1876.*

3. *Annual Reports of American Asylums for the Insane from 1840 to 1876.*

4. *Transactions of American Association of Medical Superintendents from 1844 to 1876.*

5. *Ray's Synopsis of the Laws of the several States of the Union respecting the Confinement of the Insane (Appendix, American Edition of Insanity and its Treatment.* By Dr. BLANDFORD, 1871).

6. *Report relating to the Management of the Insane in Great Britain.* By DR. WILBUR, 1876.

7. *Report of the Commission in Lunacy, Massachusetts, 1874.*

<sup>2</sup> 'Mind,' No. 1, 1876.

geny of the criminals sent to our penal colonies, and in that of respectable emigrants to new and foreign lands, and even in the capabilities of well-fed, well-clothed, well-lodged children, when associated with the waifs and strays, the weak starvelings of the poor and unfortunate. Such conditions may depend upon physiological causes upon which it would be foreign to our present purpose to enter. But, what is more germane to this investigation is that no, or very, scanty evidence exists of the prevalence of any of the forms of insanity among many aboriginal nations, untouched and untainted by civilisation, although exposed to the hardships, privations, and sources of degeneracy inseparable from a nomadic and precarious mode of life. Catlin and other travellers affirm that idiocy and deaf-mutism have not been traced among the North American Indians, even where the form of the skull has been artificially interfered with, or have, at least, escaped observation. This immunity, granting that it is real, has been explained in two ways—either, by supposing, on the faith of early explorers, that feeble and deformed infants were destroyed as burdens to the tribe, or that mature lunatics were destroyed, as is known to be the case in Australia; or, by holding that the standard of mental capacity is generally so low, and the call upon the higher faculties so limited that the harmless imbecile is regarded as an inoffensive member, if not a brave. Upon what appears the unexceptional authority of Dr. Lillybridge, of Virginia, appointed by his Government to superintend the removal of the Cherokee Indians in 1827-8-9, and who saw and examined about 20,000 Indians, particularly in reference to their diseases; who affirms that he never even saw or heard of a case of mental derangement among them. Dr. Butler, again, for twenty-five years a missionary among the same races, states emphatically that, although he had seen delirium as a concomitant of other diseases, he had never met with a case of decided alienation. He quotes an intelligent chief of eighty years of age who fully corroborated this statement. It is presumable that paroxysms of mania and of depression may have been accepted by these simple hunters and warriors as manifestations of patriotism, courage, tolerance of suffering, or, on the other hand, as the shadow of the great Spirit, and that individuals who would have been deprived of their civil rights and subjected to medical manipulation in the light of civilisation, may have secured admiration, wonder, even worship, as heroes, martyrs, saints. It is certain, further, that many moral perversions and many violations of laws recognised elsewhere, such as cunning, theft, treachery, ferocity, may have appeared to be the normal characteristics of the peoples to whose persecution and

extirpation they directly and perhaps justifiably led. The most recent and reliable information upon this point is derived from Wisconsin. The learned and experienced superintendent of the State Asylum there writes nearly as follows :

"I have made it a duty to acquaint myself with the facts concerning mental disease among the native tribes. I have never encountered more than two insane Indians, one of whom had made some advance in preparation for a missionary life, and the other had passed through the ordeal of settlement and social usages for several years. I lately made a journey to the village of the remains of three tribes near to this, in order to secure data upon this point, when their Sachems assured me that they had never either seen nor heard of an instance of lunacy, either among the present members of their families, or their ancestors. Their attention had been excited by the report that insanity had appeared in an Indian of an adjoining tribe. But it was ascertained that this person was a victim to fire-water, and not to idiopathic disease. A missionary of twenty-eight years' experience and the lay pioneers in the State confirm these facts, with the startling addition that mental disease is the concomitant, if not the consequence, of the adoption of the occupations and customs of the pale-faces."<sup>1</sup>

The Republic of the United States affords another glimpse, but little more than a glimpse, of the mental state of another division of our race when placed under trying and unnatural circumstances, or, shall we say, an experimental moral test? It would appear, from the averments of trustworthy indigenous Africans, that insanity is rarely if ever seen among natives while living in their own country and adhering to the habits of their predecessors. In whatever proportions mental diseases may have prevailed among the formerly enslaved population in America they assuredly existed and called for public provision for suitable treatment. But this is not all; a suspicion arose and obtained some currency that alienation was more frequent among free than among unemancipated negroes. This opinion, whether having another origin or not, and whether correct or not, obtained its chief support from the general census taken in 1840, wherein it seemed to be demonstrated that every fourteenth free black in the State of Maine was of unsound mind. Through the instrumentality of the eminent statistician, Dr. Jarvis, it was shown that this calculation, at least so far as it relates to the insane, was altogether and absurdly erroneous, and that there was no preponderance of nervous diseases in free over slave blacks.

Laudable efforts<sup>2</sup> have been made to collect and preserve all

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<sup>1</sup> Private letter from Dr. Kempster, 7th February, 1876.

<sup>2</sup> Bancroft's 'Native Races, Pacific States,' 1876.

accessible information regarding the past and present habits and peculiarities of the aborigines of America, both as organized communities and hunter hordes, but, it cannot astonish, that of facts illustrating morbid mental states such records are sterile. After a patient survey of the copious literature now accessible, devoted to psychology in this continent, comprehending monographs unfortunately scattered through periodicals devoted to Nervous Diseases from the days of Rush to the present time, and the thirty-two volumes of the 'Journal of Insanity,' which may be styled at once a retrospective and current epitome of what has been proposed and accomplished during the last forty years, thirty-six volumes of the 'Annual Reports of Asylums,' supplied by exchange and through the exertions of Dr. Jarvis, and the beneficent influence of the Smithsonian Institute,—memoirs which, although more pretentious, are more copious and original than the corresponding series in Britain; and lastly, the Transactions of the Association of Medical Officers connected with asylums, which constitute a historical repertory of the deliberations, interchanges of thought and opinion, and of the collective wisdom of this body during thirty years, the entire era, in fact, of the systematic study and application in the States of science and philanthropy in the relief of mental disease, fails to furnish more than the most scanty allusions to insanity as observed in negroes. This is much to be lamented, as these remnants of the coloured races, whether as labouring under innate and inheritive mental degeneracy, or under inoculated corruption, must speedily pass away, or reappear under new phases, and in the person of hybrid representatives, among the mass of the population. The same remark and regret may be extended to the silence of authorities as to any differentiation which may be detected among the elements of that composite population which, although perhaps not more varied or incongruous than our own, is less agglomerated, less fused and blended together, and still presents marked characteristics of the sources from which the various streams of immigration flowed; and this is nearly as true with regard to the descendants of the Pilgrim Fathers, as it is of the Mongolian deluge, with which the land is at present threatened. These works are likewise painfully barren of data upon which to base any perspicuous or practical comparison of alienation presented in the English-speaking races in the United States and the English-speaking races in Great Britain. We encounter the same nosology and nomenclature, founded upon psychical signs, descriptions of similar symptoms, nearly the same curability and mortality, and, so far as they are worth anything, the same pathological appearances; but it must not be forgotten that these

portraits apply equally to patients of French and English origin, who have lived under new institutions, new modes of life and nourishment, for at least a hundred years; of those adventurous, less malleable crowds, who are still rushing to the far-west Utopia, to Dutch and Germans and Norsemen fresh from their primitive and parsimonious homes,—and that we possess but imperfect guidance in enabling us to recognise precisely the more minute shades of malady under which they may labour. That the more prominent and palpable forms, at least, could still be distinguished is obvious, as there are constant confessions by American writers that many of the races enumerated are still unamalgamated with the parent stock, and are discriminated alike by race, privileges, and impending degeneracy. Thus we find the supposed increase of insanity, which, judged numerically, is much more startling than in Britain, attributed to the influx of foreigners, either because the disease is imported with them, already established in their constitution, or because they become prone and predisposed to its invasion by the new and distracting, even the improved condition in which they are placed. In Massachusetts, of 1348 lunatics, 780 were emigrants; from one asylum it became necessary to remove 175 of this class, and to refuse further admissions, in consequence of insufficient accommodation. We detect a monody over the evils of employing aliens as attendants; at the same time a confession that immigrants were especially privileged as obtaining prompt admission into asylums in virtue of their very destitution. Lastly, we have the melancholy revelation that, so far as New England is concerned, these men are more sturdy and reproductive than the descendants of the first settlers, who are now undergoing gradual deterioration as a consequence of injudicious training, fashion, and luxury.<sup>1</sup> But although the most conspicuous and desirable train of inquiry must be relinquished, the documents before us afford abundant means of determining the growth and the present standard of opinion on insanity among our fellow-labourers, the advances in medical and legal provisions, in architectural and materialist appliances for the alleviation and removal of mental disorders, and in the mode of treatment as modified by the social and political relations of the nation: to these topics we shall confine our further observations.

While it might be difficult to signalise the difference or concurrence between monomania of pride under a monarchy or under a republic, or of melancholia as manifested under the scanty fare and depressing surroundings of our agricultural labourer, and under the high wages, nourishing diet, and

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<sup>1</sup> Vide works of Dr. Nathan Allen and of Dr. Curwen, Harrisburg.

hopeful prospects of the same individual when seated under his own peach-tree in the States, we are not surprised to find that American physicians have added to the catalogue of neuroses a few species, depending, probably, upon local circumstances, and certainly unknown to us. The first of these, Typhomania, was noticed, in 1837, by Dr. Luther Bell, whose honoured name it popularly bears among the patients of McLean Asylum, generally belonging to the opulent classes, where his experience extended to about forty cases. The affection has since been recognised by many of his colleagues; but they met with examples almost exclusively among emigrants, recently subjected to all the discomforts and privations and noxious accidents of ship-board, and to the anxiety and depression very generally felt on the landing of the poor and friendless. The disease is supposed to be characterised by the abruptness of its invasion, no premonitory symptoms preceding total prostration of strength; by the extreme rapidity of its course, rarely exceeding three weeks; by the pinched typhoid aspect; by the presence of great debility and tremor; by the frequency and smallness of the pulse; by loathing and refusal of food, on the ground that it is drugged; by gloomy and confused delusions, which may for a moment be dissipated; by outbursts of fear and blind fury; by insomnia, restlessness, and emaciation; by the apparent torpor of typhoid fever, although sudamina, meteorism, and all other signs of this fever are absent, and although the faculties can be stimulated. Diarrhoea precedes death, which is sudden, the mortality reaching to three fourths of those affected. After death no pathological changes could be detected; and, what is noteworthy, the recovery is equally sudden and complete, no traces whatever of mental feebleness or obliquity remaining. It should be added that from diffusible stimulants alone was benefit derived in the treatment.

Dr. Worthington describes a variety which he has called Passive Insanity, which may coexist with any other forms of mental disorder, but which, whether simple or complicated, indicates in all cases a grave organic lesion of the brain. The diagnostic symptoms appear to be impairment or abolition of memory; the non-perception of time, place, or surrounding objects; delusions as to trade, profession, duties. However recent, such cases are incurable, and appear to us legitimately to belong to the genus Dementia.

Neurasthenia is delineated by Dr. van Deusin as allied to malarial diseases, as originating in mental labour, depressing emotions, anxiety, want, and exhausting discharges; as marked by cerebral anæmia, malaise, muscular debility, irritability, neuralgia, hyperæsthesia, depression, melancholic mania, and impaired nutrition;



as susceptible of relief from change of air, metallic tonics, and when sciatica is a prominent symptom, as is often the case, by hypodermic injection of morphia. Moreover, that genuinely subjective affection, Nostalgia, in olden times imagined to be an endemic peculiar to Switzerland, and to be the offspring of patriotism, has been observed during the late civil war.

Next to the discovery or description of a new member of the neuroses, may be ranked the record either of the introduction of a disease from the old to the new continent or its development there. When we consider that the colonization of America first began in the early years of the seventeenth century, that her independence was proclaimed precisely a century ago, and that the inhabitants have passed rapidly through numerous moral and social conditions, and have, in a great majority of instances, acquired affluence and luxurious habits almost suddenly, that they have received and are ever receiving a large increment from the excitable classes of all other lands, it is interesting to know that until 1840 Paresis was, according to Dr. Ranney, unknown, or undiagnosed, in the hospitals for the insane. This period coincides very closely with that assigned by the veteran alienist Dr. Browne to its appearance in Scotland. He states, in an address delivered to the Border Counties Association, Dumfries, that, although familiar with the appalling features of this pest in Paris in 1832, he never detected an instance in Scotland until 1839; that the increase of cases was slow and progressive, and that, even now, they are exceedingly rare in the highlands of that country. There does not appear to have been observed any peculiarity in Paresis in the United States, nor have our brethren added much to our stock of knowledge as to the malady, except, that, while the farming class is more prolific of other kinds of derangement, Paresis is still almost exclusively confined to towns; that it now presents many varieties in its incubation, duration, and symptoms. Although, unfortunately, Dipsomania and its congeners cannot be regarded as of American germination or growth, it is worthy of consideration at this stage as having received there an amount of careful investigation, and even legal interference, unknown among the races with whom it may have been introduced as a heritage. This care and importance have been chiefly shown in the creation of hospitals for inebriates, now ten in number, and capable of containing, in 1871, 6000 inebriates,<sup>1</sup> in a community where education is supposed to per-

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<sup>1</sup> These individuals were consigned to brief seclusion by the following modes and in the following proportions:—Committed by mayor for specific period, 214; by process of examination by judge and jury, when curator and confinement adjudicated, 144 by voluntary act 5515.

meate very deeply and widely the different strata of society, where drunkenness is said to be less blatant than in our large towns, and where nearly the same facilities offer for the admission of the involuntary drunkard into asylums. Such establishments, whatever may be their demerits, betray the existence of a social want, or of speculation founded upon such a necessity, and are, of course, the exemplars or imitations of shelters in Britain, improvised upon the same principle, but less pretentious and less trusted. They possess somewhat of the recommendations of a club-house or hotel, where a voluntary penitent may reform himself, but of their curative or deterrent powers we are sceptical. This doubt is suggested mainly by the facts that, although medical men in the United States very generally adopt Dr. Woodward's definition, enunciated in 1833, that dipsomania is a physico-moral disease, and presents the following symptoms—sudden incursion; hereditary tendency; paroxysmal character; destruction of the sense of shame, of right and wrong, of property and propriety; occurrence in the pure, abstinent, even in the ascetics; convertibility into other forms of alienation—they as generally withhold their countenance and confidence from these homes; and, secondly, the boasted success of from thirty-five to fifty cures per cent. in the most intractable, and, elsewhere, incurable Craving, inspire the suspicion that the recovery from a fit of intoxication has been mistaken for the eradication of the impulse which leads to indulgence. This suspicion is corroborated by the mode in which Dr. Willard Parker essays to establish a ratio between the number of discharges of cured inebriates and of cured lunatics. We would willingly dismiss as exaggerations the assertions that these Retreats serve rather to conceal the acute stage of Delirium Tremens, or the degradation of confirmed sottishness, that stimulants are accessible and used within their penetralia, and that the police are sometimes required to restrain the extravagancies of excitement. It is quite evident that some provision, some restraint, is demanded to meet the exigencies of wide-spread Intemperance; and because the medical officers of asylums have declared, we think wisely, that inebriates should not be associated with ordinary lunatics, the Association has recognised this exigency by declaring that hospitals for dipsomaniacs should be provided by the State, to which such insane drunkards should be committed by law. Moreover, the benevolent and pious portions of society are divided and disturbed by a controversy on the question whether habitual drunkenness be a voluntary and vicious indulgence or a diseased and uncontrollable appetite; but, in whatever manner settled, the opinion of the alienists stands good that a distinct asylum is required for Inebriates.

This contention, which threatens to agitate our own community, obviously involves all the bearings of Moral Insanity, for, if a solitary instinct may become diseased and so irresistible as to regulate conduct in spite of intellect and conscience, there must exist a class of perversions altogether independent of madness, as generally recognised. Dr. Ray argues in favour of this theory. He cites Pinel as describing cases of derangement without lesion of the understanding; the modern admission of disease of the moral sentiment, while the intellect remains healthy; then defines the affection as perversion of moral powers, changes in character, loss of the finer sensibilities, narrow moral obliquities, complete inversion of the perception of good and evil. He admits that, although this deviation from health may overshadow all the other mental powers, and become so pronounced and prominent as to be denounced as depravity, there are cases where such deviations are effectually concealed by cunning self-control, or capacity still exercised by the unimpaired faculties. These statements are controverted by his antagonists by an admission of the facts, a denial of their interpretation, and an attribution of the mental phenomena to evil propensities for which the actor is responsible; by a protest against the abstraction, disease, being the impelling force to certain unusual or criminal courses, and by a call for examples of the compatibility of emotional proclivities to evil with intelligence and general propriety of conduct. This call is answered by Dr. Woodward. A girl, aged 15, of pleasing countenance, modest and correct in demeanour, was placed in his asylum upon two occasions, her seclusion extending in all to fourteen months. During this long period her conduct was rational, industrious, and in all respects exemplary, so that her attendants regarded her as a sane and most useful assistant; but, whenever the patient was restored to her parents and home, she became sullen, disobedient, pilfering, destructive of furniture and dress, cutting from her frocks patches of cloth of the same size and symmetrical in position. These inexplicable acts occurred repeatedly.

Apart from the irreconcilability in certain minds to admit that as madness where there is neither folly nor fury nor incoherence, many of our brethren recoil from such an admission, on the grounds that it is impossible to conceive, especially where responsibility is concerned, that morbid action can affect one isolated and insignificant emotional feeling, while all the other mental powers continue healthy and robust; that disease can be so fugacious and so slightly detrimental in its effects that it may rule for an hour or a minute and then pass away, leaving no trace, and perhaps permanently, as when reason is said to be suspended

at the moment of the commission of a crime, or that foul and loathsome lusts and longings, or gross and grotesque illusions can find tolerance in spirits otherwise consecrated to virtue or devoted to the exercise of the higher faculties in science or art. These obstacles or objections are very discernible in disquisitions and discussions upon Mania Transitoria, on which Dr. Jarvis is an authority, in the important memoir on Impulsive Insanity and the Homicidal Tendency written by Dr. Hammond. The views of the latter writer do not find much favour with his countrymen, as, in discussing unprovoked murders, perverted affection, and blood-thirst, he advocates the punishment of the offender chiefly on the plea of justice to society, and that he is competent to know the effect of a poignard, a pistol, or garrotting. A very large number of their juri-consults—especially Dr. Ray, whose work on the medical Jurisprudence of Insanity is the best on the subject in the English language—advocate a more humane and enlightened course, and would shudder at the possibility of contributing to hang a lunatic by mistake, or because he was dangerous or in virtue of a metaphysical crochét. Every precaution is adopted to protect the criminal who is, or pretends to be, of unsound mind, even those accused who occupy the debatable land of comparative sanity; and the leading mind in this department has, during many years, put forth sound and safe advice for the guidance of those upon whom devolves the painful duty of giving testimony which may be tantamount to a deprivation of life or liberty. He recognises the admissibility of Experts in criminal cases, warns them against system and system-mongers, and likewise against the difficulties and delusions suggested to them by Counsel. He recommends the avoidance of particular indications; that the whole history, conduct, and conversation of the prisoner should be embraced, even the delicate shades of disposition which can only be detected through long observation of mental and moral disease, in conjunction with a comprehensive knowledge of the healthy mind. He protests against reliance on definitions of insanity, against dealing with supposed cases, solitary symptoms, with the possible identity of crime and insanity, with any consideration except the evidence produced in court, upon which whatever opinion is formed should be expressed in scientific and modest terms. As Moral Insanity is still a *questio vexata*, it should be avoided. While it is contended that the co-operation of Experts in such trials is absolutely requisite, the mode of their assistance is undecided. Dr. Ray is disposed to stigmatise mere *vidé voce* deposition as vicious; he even conceives that personal examination of the accused, made as it must be in the presence of jailors and strangers, may fail; and that a trustworthy opinion

may be reached through written testimony, or, as has recently occurred, through the evidence of witnesses heard in court. He treats as irrelevant the notions that experts are blunted to the minor shades of psychical unhealth, that they are predisposed to see disease in every erratic act; contending that by their experience alone can such slight perversions be discovered when concealed by the superficial intelligence, adroitness, or cunning of the culprit. He is doubtful as to whether medical witnesses should meet for consultation previous to their appearance in court; but this natural embarrassment has been summarily and satisfactorily settled by the Legislature of Maine, which has enacted that persons stated or suspected to be insane are to be examined in an asylum previous to trial by medical men engaged in the study and treatment of alienation, who are not to be selected by the counsel on either side, and whose report is to be accepted in lieu of oral testimony in the witness box, and in lieu of that partisan contention and advocacy and hard swearing which we are sometimes accustomed to lament, and which is perhaps inevitable when medical men are placed in the position of counsel and engaged to refute or defend certain propositions, and, according to Dr. Ordonaux, "drive a profitable trade." We regard this as a noteworthy and laudable advance in legislation and the application of science, a triumph over prejudice which we cannot yet boast of as secured in England, and which has not yet been accomplished, so far as we know, in any country but Germany and France, where the suspected may be placed in an asylum for prolonged observation. Dr. Stokes gives expression to the sentiment of many of his collaborators in advocating the adoption of this law by all States of the Union, under the modification of a court or commission of medical experts, who should determine the validity of the plea of insanity previous to judicial proceedings, his grand object being to remove the difficulties experienced by juries when conflicting medical opinions are submitted, and to prevent the issue being dependent upon the statements of unskilled medical men who are summoned merely to represent the views of counsel. We have heard of an improvised remedy, where a superintendent was appointed judge advocate and endowed with military rank, and instructed to proceed to a distant State, in order to decide on the mental condition of a medical man who had shot an officer, and likewise on that of a soldier who had tried to poison all the inmates of the guard-room, and succeeded in two instances, with morphia, in order to effect his escape. In both cases the prisoners were found sane and we believe sent to the scaffold.

What may prove a severe aggravation of the punishment of a condemned sane prisoner and must serve as a salutary protection

to an improperly condemned lunatic prisoner, is to be found in the provision in the United States law, that the execution of the sentence of death is not to take place until a year after condemnation. There is naturally considerable discrepancy in the convictions of American alienists on many of the phases of criminal insanity; in fact, they occupy nearly the same battle and border grounds as we do; but there prevail among them a perfect unanimity and harmony as to the legal position and destination of such offenders as have been pronounced by the law irresponsible. They protest indignantly against the confinement of such unfortunates in jails and penitentiaries, associated with the vile and the vicious, even with convicts, a practice necessitated by insufficient accommodation in asylums; even against their detention as insane with lunatics in almshouses and county hospitals, where the arrangements are wretched or injudicious, and where, from mismanagement, curables are converted into incurables. This protest has been embodied in appeals to the different legislatures and supported by all the corporate influence of the Association of Medical Officers connected with Asylums. This movement has borne fruits; institutions have been erected for this class, as at Sing Sing, Auburn, New York, and are contemplated in Pennsylvania; small succursal hospitals have been provided in connection with prisons, so that the partial isolation of the class has been practicable; and where no positive action has been taken the attention of legislators and statesmen has been aroused and swayed and the sympathies of the benevolent and educated have been engaged on the side of humanity and common justice. American physicians are likewise fully agreed that this class should not be introduced into asylums for the ordinary insane, and they likewise concur in the recommendation, still under consideration in this country, that there should be a broad distinction drawn and acted upon between criminals whose acts were the suggestions and symptoms of insanity and those whose insanity was the legitimate outcome and end of a course of vice and culpability. This tendency to subdivision and segregation of the mass of the insane population appears to be a characteristic of psychological opinions in the United States. As in England, soldiers and sailors are placed in special asylums; Quakers and Roman Catholics have provided distinct but not exclusive retreats for members of their communions, and it is easily intelligible that a separation of white from coloured patients under treatment should be expedient; but we were not prepared for the proposition that the two sexes should occupy different asylums, or to hear that the principle had actually been carried into effect. We knew that Dr. Storer, under the title of 'A Gyn-

acist's idea of *Insanity in Women*, had proposed the distinct study of their constitution, function, and mental failures, but knew at the same time that his conception had been ridiculed as mono-ideaistic, and on the ground that he would observe such failures through the speculum, while an alienist would do so through the manifestations of psychical disease. Dr. White has, however, furnished an hospital, for the prosecution of the speciality, at Hudson. And further, it would appear that a section of Theorists, actuated by the tendency mentioned, have sanctioned the construction of separate asylums for the Chronic Insane, as those of Tewkesbury and Willard, a retrograde movement finding a parallel here in the formation of the vast *Dépôts* for Dements and Dotards at Leavesden and Caterham.

But this step was unquestionably in opposition to the mature views of a large portion of the profession. This separation has been strongly objected to, even by superintendents whose establishments were crowded to excess with an accumulation of cases of long standing, and where the demand for the reception of patients recently and acutely affected was clamorous, upon the grounds that asylums are not merely curative, but conservative and protective; that chronicity was not a proof of incurability; that the presence of docile, long-domesticated, partially demented inmates was a positive benefit in respect to service, industry, example, and in moulding, softening, subduing the more vivacious, refractory, and untrained members of the community; that the same staff, social arrangements, nearly the same space, suffice for chronic in conjunction with acute cases; that it would be unfair to subject the former to a more spare because more economical regimen and accommodation, in keeping with the popular notion that a strong room at home, and a box or cage made by the village carpenter, were all that a sexagenarian victim to fatuity could require or desire.

An imperative cry has arisen for the appointment of a commission to adjudicate in this conjuncture, to decide as to the probabilities of recovery in these different classes, and to negotiate with county authorities for the accommodation of each. The unanimity of this appeal can be more confidently asserted, as we find in the meeting of the Medico-Psychological Association in 1868 that the following resolutions were recorded:—1st. That the State should provide accommodation for all its insane; and, 2ndly, that the curable and incurable should not be separated. This consensus is arrived at by a society resembling that bearing a somewhat similar name in this country, embracing all superintendents and medical assistants, which assembles yearly for a session of about a week, spent in reading and dis-

curring papers and in visiting public institutions, and whose 'Transactions,' published at considerable length, and after careful digestion and expurgation in the journal, and in an epitomised form as a separate volume, may be accepted as an expression of the deliberate opinion of the practical men in this speciality.

It is obvious, that to draw a sharp and workable line between hopeful and hopeless cases of madness would be impossible, and, as might be expected, there are consequently encountered regrets from physicians over the unavoidable discharge of chronic patients, sometimes amounting to hundreds; over the premature removal of others by friends; over the occurrence of relapses and readmissions; over the confounding of paralytics and dipsomaniacs with those who are really improved, and impressive injunctions in favour of prolonged and probationary convalescence in asylums, on the ground that a speedy cure is creditable, a permanent cure a triumph. From such practices, from the increase of nervous diseases, the greater confidence reposed in public treatment, and, notwithstanding gigantic efforts made to afford accommodation, arise, according to Dr. Jarvis, private speculations, which are not, however, in keeping with even the genius and taste of the affluent inhabitants; not above five asylums of this kind, capable of containing a small number of inmates, are known in the Union. It is interesting to contrast his calculation made in 1858, as to the relative numbers of such institutions in America and in England. Of 45 asylums in the former, 5 only are private; while in the latter, of 165, 113 are private. Of the 45 here designated 27 have been provided by twenty-one States, 5 by respective cities and counties, and 10 are corporate institutions. To each of these, whatever its destination may be, there are attached invariably trustees, one physician, supreme in power, generally resident,—in one instance, at a former stage of progress, four were employed; sometimes a consultant, one or more medical assistants, and recently in Utica Asylum a pathologist has been added to the staff. The duties of the officer last named are:—1st. Examination of secretions. 2nd. Of pulse by sphygmograph, in relation to mental condition. 3rd. Of pulse under administration of medicines. 4th. Examination by ophthalmoscope. 5th. The skin, its conditions. 6th. Post-mortem appearances. 7th. Photographic representations of such appearances. In keeping with American institutions, but sadly inconsistent with our notions upon the subject, the medical staff is removable in certain States on the occurrence of a change in the political constitution of the government.

Estimated by their work and their writings, American alienists consist of educated, experienced, and enlightened men. Their



ranks are supplied with trained recruits. After some discussion as to the mode of proceeding, it has not only been universally acknowledged that no medical curriculum can be complete which excludes the study of mental diseases, both systematic and clinical, but in several schools—Harvard, New York, Philadelphia—regular lectureships have been created for such tuition, following the example of the celebrated Dr. Rush, who gave clinical instruction on the subject in 1805. By such instruction, after ascending through the grades of apprenticeship in asylum organization and working, the alumni are prepared for independent appointments throughout the Union.

To the complement of apothecary, chaplains, matrons, stewards, to which Englishmen are accustomed, is superadded, as a minimum, one attendant to ten patients, or two or more as may be expedient. This liberal proportion is not a legal provision, but the recommendation of the Association; but it may have, in some measure, led to the scarcity of the materials out of which such officials are formed, and to the employment of aliens, prisoners, convicts, and other unqualified or disqualified castes, as attendants in county hospitals and almshouses. Dr. Curwen, in Pennsylvania, states that he has no difficulty in securing suitable assistants; but Dr. Gray, in New York, who encounters the same obstacles as are presented in this country, constitutes his corps by causing the applicants to run a sort of gauntlet, by weeding, watching, giving good wages, and retaining the trustworthy, or, at most, the least objectionable. It is palpable that in the United States the muscular, the moral, the mental qualities which are desirable in these the most important co-operators in moral management, the strength to interfere, the humanity to forbear, and the good sense to discriminate, are wanting or rare, so that the excitement, the insubordination to non-official authority which is said to be characteristic of American lunatics,—which is easily intelligible in a country where equality prevails, and where submission is yielded exclusively to the mandates of the executors of law,—must often be beyond control, and even beyond the majesty of numbers. We have before us the evidence of a superintendent who, failing in transforming mere hewers of wood and drawers of water into gentle, judicious, self-denying agents, first engaged self-educated and thinking artisans, but they turned out philosophers; secondly, pensioned soldiers, but beer proved more fatal than the enemy in the field; then communicants from the several churches, but their religious impressions failed to regulate their temper or passions; and, lastly, he organized a school, in which he taught, lectured, and illustrated, happily winning greater success than had crowned his previous

attempts. Dr. Stribling, in enumerating sobriety, discretion, uprightness, subdued temper, perseverance, firmness and decision to carry out,—even by force or prowess, medical orders, pride in relieving suffering, facility in devising occupation and amusements, and faculties and feelings which should endow a master as well as a companion, has “portrayed a monster that the world ne’er saw,” a model and standard attendant, and who,—seeing that many of the delineator’s brethren covet the power possessed in England of prosecuting for the maltreatment of patients, must still be in the far future. Our brethren have discouraged the services of females, even in the male wards of hospitals, and have not tried the experiment of employing married couples in such galleries as offer opportunities for such an association. The only remedies for the defects complained of seem to be promised in a scheme, still inchoate, promulgated by Dr. Yellowlees, of Glasgow, for instructing and training clinically, and endowing liberally, a superior class of supervisors; and in a school for nurses now actually in operation in the Charity Hospital, New York, under the guidance and governance of Dr. Kitchen, formerly of Utica Asylum.

Next to moral and manual means indispensable in bringing the insane under either moral or hygienic influences is mechanical restraint. One and all of American alienists concur in believing that coercion is a powerful adjuvant, in itself a moral instrument, and indirectly required in the application of medical remedies for the restoration of bodily health: and for the preservation of life. This general accord was confirmed by a vote of the Medical Association in 1874. While these scientific men are unanimous as to the propriety, or expediency, or usefulness of physical restraint, they differ widely as to the reasons and circumstances demanding its application, and as to the extent to which it may be carried, many of its advocates scarcely resorting to it at all, and others resting upon it as a frequent and potent aid. This creed does not harmonise with that formerly universal in Britain, but, as was shown in an article in this *Journal* lately, now accepted with a less rigid and more relaxed orthodoxy, and viewed in some quarters with scepticism. Non-restraint became the watchword of a party—of a persecuting party, which denounced all who rejected allegiance, all who preferred a thong to a threat, the embrace of a camisole to the hug of rough and determined hands,—as cruel, unconscientious, and as incapable of appreciating the principles of medicine or the dictates of humanity; therefore we disliked it. That such a resource can be dispensed with is perfectly true, but so can medicine, as is done by certain nullifidian physicians, whose practice, if not their profession, is limited to fresh air, good

food, and amusement run mad; but what we chiefly object to is, the denial of fellowship and sincerity to those who differ from us, the reluctance to admit that they should pursue a mode of practice inconsistent with our own, and that they are not actuated by the same high motives and by the results of an experience as wide as our own, though differently interpreted. The calm, dignified, pacific rebuke with which American alienists have met such insinuations should be compared with the harsh criticisms which are still directed against them. We lately conversed with a superintendent who, led, perhaps awed, by the example of Conolly, never resorted to restraint, whose career has nearly reached that crisis where our professional as well as our personal errors come to be reviewed and repented of, and whose concluding sentence was—"Three things I bitterly regret—1st, that I trusted too little to stimulants; 2nd, too little to opium; 3rd, too little to restraint."

It may be interesting to record, as showing the great revulsion in medical opinion on these points, that in 1840 in the Asylum of Maine, that State which has assimilated its legal code in relation to Criminal Lunatics with that of France, twenty-nine patients were handcuffed and forty chained, a procedure resembling that observed in the Salpêtrière in 1832, where eighteen old women were confined or strapped in *chaises de force*; and in our own country in 1844, where the Metropolitan Lunacy Commissioners described the liberal use of leglocks and hobbles, and chains and fetterlocks.<sup>1</sup>

It has been supposed that the absence of due labour involves the presence of undue restraint. Dr. Ray, in 1865, appreciated the importance of occupation to the health and happiness and recovery of his patients; his experience had convinced him that, although of great moral, it is of little pecuniary benefit; that, by multiplying attendants, out-of-door labour can be indefinitely extended, although numerous patients are unfitted by habit, trade, illness or exhaustion for such exertions, and others prefer, or, are best capacitated for, household duties and handicrafts. He so succeeded with the community under his charge as to bring about one fifth under the operation of this powerful agent.

Dr. Gray, New York, calculates that, after making the required deductions for age, sex, illness, and incompatibility with the form or stage of the mental disease, his industrial corps would amount to 25 per cent., although his annual reports reduce the proportion of actual workers to 18 per cent. In the institution over which he presides it is asserted that workshops and schools initiated by his predecessor, Dr. Brigham,

<sup>1</sup> 'British and Foreign Medico-Chirurgical Review,' vol. li, p. 305.

have been abandoned. Dr. Wilbur, who has advanced this statement, has recently visited this country for the purpose of reporting to the Board of State Charities the results of his examination of a number of British asylums. He produces rather a sensational effect by placing the 68 per cent. of patients employed, of the 9786 seen, in contradistinction to the happy idleness which he attributes to the inmates of similar hospitals in his native country; by encomiumising the tranquillity, docility, and contentment of the inmates, the beauty and ornamentation of their abodes, and the non-existence of physical appliances. Yet one American superintendent values the remedial effects of labour so highly as to propose that it should be made compulsory on patients. The Doctor's facts are of course inexpugnable, but his impressions are derived from a few selected, celebrated establishments, and are contemplated through an atmosphere so *coulour de rose* that an Englishman standing by his side, but embracing the whole field of vision, would scarcely recognise the picture, and might be inclined to look forward to such havens of rest as a premium upon folly and a solatium for all the ills that life is heir to. That all lunatics may be, after a fashion, engaged in work, can at times be taken into the open air, can be indulged in an almost unlimited amount of freedom, has been demonstrated; but the inquiry arises, is such latitude beneficial? Restrictions to the sane mind prove necessary moral checks; active exertion proves a bane as well as a blessing in different cases, and it should be recollected that Guislain condemned more emphatically than the Americans, toil and travail and muscular activity, as inducing hyperæmia in all the tissues, or, in modern phraseology, nervous excitement.

When it is considered that the theme of almost all physicians and philosophers in the States has been that insanity is a bodily disease, that it owes its origin in a far larger proportion of cases to physical than to moral causes, that no case of mental disease can be examined where organic changes are not discovered, and no necropsy performed without the detection of conspicuous structural degeneration, it can readily be understood that the therapeutic means of restoration adopted have been very numerous. It is true, that, latterly, greater attention has been paid to diet and regimen, that the efficacy of abundant and nutritive food has been generally recognised, and that the necessity for alimentation, natural or forcible, is nearly as justly estimated as in Britain. It may be recorded, parenthetically, that so lately as 1859, Dr. Chipley seems to have lost a patient by inanition, having placed reliance upon persuasion, stratagem, deceit, example, tempting viands and energetic remonstrance, rather than resort

on the prompt use of the simple instrument recommended by Dr. Hamilton, or the various appliances invented in Europe.

On our first acquaintance with the therapeutical means associated with seclusion and hygiene we came into contact with bleeding, drastic purgatives, blisters, opiates, tonics, so generally trusted to at the commencement of the present century, but as we lived on a different class of remedies obtained popularity and confidence. Depletion may not be regarded in the States with the same unfounded horror as here, but the arguments and experience upon the subject were fully placed before his confrères by Dr. Pliny Earle in 1854, in a treatise directed against the teachings of his sanguinary but celebrated countryman Dr. Rush. Opium is still much trusted to, but bromide of potassium and chloral divide its reputation, and Dr. Ray has recommended etherisation either as an aid or a substitute. He ventured on this step notwithstanding the failure of the process in other hands and in consequence of the failure of other drugs in his own hands. When exhibited at night he found that it induced natural sleep after the first sopor, and established tranquillisation and rest in cases of nervous restlessness, of panphobia, and of suicidal tendencies. Chloroform appears to have been given in mania and puerperal mania, but whether the result justified a continuance of the course we are not informed. Nitrite of amyl has, very recently, been used by inhalation and with great benefit during the status epilepticus, by Dr. Kempster, Wisconsin. Of the drugs which have lately attracted attention, or secured general acceptance, the following catalogue may suffice:—Ergot, oxide of zinc, muriate and formiate of ammonia, gallic acid, Indian hemp, phosphorus and phosphoric acid, and tea, in coma.

It is probable that the thoughtful men have at length discovered that the physical or materialist theory fails as lamentably as the psychical to explain the cause and the phenomena of derangement, and the connection of these with nerve-cells. At all events, it would appear that the champions of organization as the origin of spiritual life—Gray and Ray—have found antagonists, disciples of Heinroth, in their own chosen field, and that their postulates are no longer to pass undisputed. In a recent criticism by Dr. Wilbur on the propositions enunciated by Dr. Gray, that insanity is a disease of the brain, or of parts of the body acting upon the brain, it is retorted that the phenomena of consciousness are more essential elements of mind than the impressions of the external senses, that "I cannot see thinking, but I can think without seeing;" but Dr. Wilbur

concedes the questions at issue,—in cant language throws up the sponge in the controversy, when he confesses that consciousness depends upon molecular movements.

The modification thus presumed to be going on in philosophical opinion may have conferred that prominence upon what is rather indefinitely designated moral treatment, and which consists in appeals to the religious sentiments, the social instincts, the artistic tastes, and the craving for recreation and stimulation which marks the present generation. Such appeals have been introduced as positively curative, and consist of all the appliances utilised by psychologists in Europe, in surrounding the institutions with gardens, grounds, and beautiful prospects; in converting the wards into picture galleries; in the use of music, games, gymnastics, and amusements of every conceivable description, from lectures and literary soirées down to nine-pins, and all these provided in such abundance or superfluity as to suggest satiety rather than enjoyment; and in some institutions, as in that of Philadelphia, where there is an assembly of patients for some beneficial or enjoyable purpose every night, which exceeds the most extravagant developments in this direction elsewhere. In the Annual Report for 1876 it is stated, that, during the space of seven years, there has been an amusement every night for nine months of each year, that light gymnastics have been practised as constantly during eleven years, that not merely has fancy wood sawing been introduced, but that a school for cookery is about to be instituted, as occupations for the higher classes, the latter resource being, we believe, a decided and valuable innovation.

So early as 1831 such amplifications of moral discipline, with the addition of plays, tableaux, theatrical exhibitions, debating societies, and fairs, were fully appreciated and resorted to in New York Asylum.

The observations of American pathologists, although not positive discoveries, and nearly coincident with those recorded in the old world, are worthy of note. Dr. Hun announces that he finds the diagnostic pulse of uncomplicated cases of insanity dirotic, or monocrotic, not tricrotic. The presence of syphilis and phthisis in alienation is recognised, but rather as complications than as symptoms of a specific disease. Locomotor ataxy has been long recognised and fully described. The connection of chorea with the rheumatic diathesis, of herpes with nervous affections, of hysteria in youth with insanity, and the supposed evils arising from intermarriage of relatives, have all been more prominently considered than in this country. One lay philanthropist has gone so far as to declare, in reference to the last evil named, that the prohibitions of such unions would

diminish by 20 per cent. the blind, deaf and dumb, idiots and lunatics.

Up to a certain point the structural changes in the nervous tissue noticed are precisely the same as those which now burden our systematic treatises, including thickening of skull and meninges, effusions, softening, hardening, tubercle. The microscope has also been resorted to, and Dr. Gray reports alterations of brain substance in fundamental structures, in the neuroglia, in the increase of the interstitial amorphous matter, multiplication of connective fibres, diminution of connective nuclei. These features he finds chiefly in the anterior portions of the brain connected with capillaries, and such degeneration to be localised chiefly in cystic cavities. He insists much upon granulations not being fatty, adverts to correspondence between degenerations in grey and ganglionic matter, to the changes occurring round the arterioles and the fatty propagations to adjacent nerve structure, to the perivascular canals, and all those departures from supposed healthy brain with which microscopists have of late become familiar. His first observations were confined to one hardened brain from an acute maniac, but his subsequent essays have extended to fifty-two brains, including those of maniacs, melancholics, demented, paralytics, &c. Very beautifully executed micro-photographs of these researches have reached this country. Dr. Gray admits that changes in the third convolution are not necessarily connected with aphasia, and that the Gruyère appearance, or broken connections in cortical matter cannot be identified as the basis of hallucinations; but, alas! what he has so carefully observed are proofs merely of disease, but not of any particular mental disease, and we must pause before venturing upon any conclusions as to the influence of cerebral destruction or deterioration, as we learn, while writing, that one of our most accurate microscopists is now convinced, that appearances similar to those seen in brain wasting, paresis, and so on, may be traced in the brains of those free alike from mental and bodily disease, and that many of these changes are connected rather with age than discernible constitutional affections.

In tracing the history of American psychiatrics we are constrained to regard them, not as offshoots or branches from our parent stem, but as a part and parcel of ourselves. Brethren inhabiting an adjacent region, somewhat different in climate, natural productions, and social polity, but who have passed through similar courses, cataclysms, tedious and tiresome labours and lustrations; who have participated in our errors, excellencies, principles, and prejudices; who have met with the same obstacles, epochs, resting-places in their progress, and

who have reached, not perhaps a strict community of sentiment, but a close approximation in the estimate of the grand interests at stake.

The important event of founding the first asylum in the United States occurred about 1745. It occupied the site of the present City Hall in New York, and was destined for the indigent poor, the sick, the orphan, the maniac, and the refractory. In 1791 the New York Hospital was erected, appropriated likewise for those labouring under bodily as well as mental disease, a conjunction which was continued in this country until 1840. In 1808 a noble building, called the Marine Department of the New York Hospital, was erected, and seems to have been the first public institution consecrated to the exclusive admission of the insane. Asylums of various form, size, structure, and pretensions were subsequently and in rapid succession constructed, and Dr. MacDonald was sent in 1831 to Europe, in order to inspect and report upon corresponding buildings. A more prolific visit was paid by Dr. Luther Bell in 1845. He was commissioned by Government, and animated with a clear conception of the objects in view, and with a conscientious estimate of the deficiencies which it was desired should be corrected. He returned home deeply impressed with the facts that the present grandeur, spaciousness, and accommodation of the asylums then in use in England contrasted strongly with the mean, miserable, antiquated buildings which they had superseded; that the latter had been sold for workhouses, and that the age of cheap provision for the maintenance of lunatics had passed away. The result of his observations was a plan for the construction of Butler's Asylum, Rhode Island. From this period, and for a long time thereafter, the characteristics of American resembled those of English asylums; in fact, reproduced what Dr. Bell had observed in his tour, and may be epitomised as comprehending, architecturally, a right line with projections at ends and at centre; internally, broad galleries, with rooms on one side, windows on the other; associated dormitories, private parlours, and sleeping rooms in different positions; bedrooms for single patients, amounting to a half or a third of the whole number; public rooms with open fires; attendants' rooms to have clairvoies, and to be so situate as to command view of sitting saloon or dormitories; the house to be of three stories; the day rooms below, the bed accommodation, &c., to be above, to be distributed according to the condition, &c., of the inmates; the apartments for the violent to be below and behind, with conveniently arranged baths. There was to be besides a large, well-furnished public hall, and the whole structure was to be heated and ventilated by air passed over furnaces, entering



through tubes and ventilators; the principle being that of getting large quantities of moderately heated air to penetrate to all parts of the structure, in place of trusting to small quantities of overheated air, and so on. Whether air, steam, or water be used, the heat is, generally, diffused by radiation from metallic plates, and encouraged to reach about 70°, a temperature in keeping with the domestic habits of the inhabitants of North America, and, although greater than that permitted in our public institutions, does not exceed that prevailing in manufactories and public works where health and longevity do not appear to be affected by this course. In fact, representatives of such structures are still to be found throughout Britain. In 1847 the Hospital at Utica, now presided over by Dr. Gray,—well known in Europe both as a philosopher and a pathologist, is described as one mile in internal extent, though every point is accessible in two minutes, and as capable of containing 430 patients. It boasted of such advances as the possession of a sick ward, of a separate chapel, museum, picture gallery, and, what was equally important, improved waterclosets. It must not be forgotten that coeval with these and other ameliorations, there were, as there still are, almshouses containing, according to Pliny Earle, 35,000 lunatics, then as now, described as revolting and disgraceful in filth, wretchedness, and mismanagement. At so remote a period as 1807, even before the revelations of similar scenes with us, 300 raving or demented beings are reported as subjected, in these abodes of misery, to cruelty, privation, and gradual death. In 1830 Horace Mann, and in 1842 Miss Dix, endeavoured to rouse the public conscience to the investigation and eradication of such atrocities, and so recently as 1863 the Commissioners in Massachusetts report—

“Under the inclined roof of an outbuilding connected with a poorhouse, a situation where the inmates must have suffered intensely from cold in winter and heat in summer, the Commissioners found a man and a woman confined in what were in fact cages, on the opposite sides of a narrow passage-way that led to a small window in the gable-end of the building. There was no ventilation, and the walls and ceilings were daubed with excrement, and there it was understood these captives had been immured for years.”

“In another filthy cage the same officials found a man who had lost most of his toes and the half of one foot by intense cold and neglect.”

A witness from Scotland testifies, in 1868, to insufficient space and air, to crowding, to the rare use of whip, to the cages, and to chains for the violent.<sup>1</sup>

<sup>1</sup> Dr. A. Robertson, Lunatic Wards, City Poorhouse, Glasgow. Reprint, ‘Journal of Mental Science,’ April, 1869.

Against such horrors—nearly equalled, however, by shocking narrations contained in the report of the Royal Commission in Lunacy, Scotland, and in the first volumes of the Scottish Commissioners in Lunacy—the cry of indignation is so universal in the States, and the struggle to provide adequate accommodation so earnest, that they may almost already be numbered among the plague spots of the past.

In Pennsylvania, where a hundred years ago the care of the insane was extremely parsimonious, there was constituted in 1796 one of the earliest refuges for this class. In 1836 the asylum was extended and improved. From the commencement until 1854, of 7726 patients admitted 3149 were cured, and of the 3360 admitted in the last thirteen years, 363 died. This asylum, by common consent regarded as the best conducted, if not the best constructed in the States, is surrounded by a well-wooded park extending to 111 acres, which, although the City of Philadelphia has touched and almost embraced it, contains farm, gardens, kitchen and ornamental, and all the provisions for out-of-door exercise and recreation. It is under the superintendence of the accomplished Dr. Kirkbride, long famous on both sides of the Atlantic, from whose sketch of what already exists, and of what Dr. Pliny Earle has designated the Psychopathic Hospital of the Future, we may cull a few particulars. He conceives that the treatment of the insane is facilitated or impeded by the nature of the dwelling which they are compelled to reside in; secondly, that the gross number of inmates should not exceed 250, although, in reality, many asylums contain nearly 1000; thirdly, that State hospitals should be provided for all the insane, that the classification should be according to station; fourthly, that the walls should be of stone, or stuccoed brick painted, that arched passages and iron doors should protect against fire, that roofs should consist of copper, tin, or slate, that there should be an hospital-room for each sex, that food should be circulated throughout on hot plates by railways, that the circumvallation should be concealed and not of such a height as to obstruct the view of the external world. He is opposed to open fires, but proposes that heating shall be effected by steam; that conflagration may be met by attaching hose to the water plugs and by using the engine as a forcing pump, and that communication shall be established by bells and speaking tubes. He advocates the institution of teaching, night watching, and the compensation for work or assistance rendered by patients; and although confessing that restraint and seclusion cannot be abolished, he denounces the superfluous means and signs of confinement, suggesting the removal of bars, double window-

sashes, &c. He recommends the improvement of land as a mode of treatment, has forty-one acres as pleasure ground, the remainder under cultivation; that the walks and drives should be formed of brick, gravel, tan, to secure dryness. That separate cottages should be studded around but connected with the main building by covered ways, that there should be airing grounds for privacy and special cases, that there should be conservatories and summer-houses scattered around, and that the residences and domains for the two sexes should be separated by an empty space, by shallow canals or ponds, or, as in his own case, by a deer park.

In order to relieve from the accumulation of chronic patients, and as a mode of management, the attachment of cottages or small separate pavilions or houses has been proposed, but in very few instances carried into effect. As might be anticipated, no general plan has been pursued in constructing asylums. The counsel of experts, the caprice or fancy of architects, the laws, the finances, the fashion of individual States, have led to great diversity in form, and the repeated visits of artists and psychologists to Europe have failed to introduce either uniformity, or imitations or improvements of the best models. In the absence of any central regulating body an important step has latterly been taken by submitting the plans of new asylums to the consideration of the Psychological Association. Either through the influence of this society or the energy of certain of its members, such as Nichols and Gray, several new edifices are in preparation in accordance with what may be styled the most approved principle in this country, that is to say, blocks of two storeys connected by covered passages, with great amplitude of space and provided liberally with bays and glazed bow windows. The claim of this body to instruct and regulate public and legislative opinion upon this and cognate subjects has been advanced since its commencement in 1844, when a committee was appointed to report upon the construction of asylums, and asserted throughout its course, as in 1851, a series of resolutions, among which were the following:—That hospitals for the insane should be in the country, surrounded with from 100 to 50 acres of ground, abundantly supplied with water; that the plan for not more than 250 be approved of by experts; the house to consist of eight distinct wards, to be built of stone or brick; that each subdivision be arranged in the same manner as Butler's, no apartments being subterranean, and none unprovided with windows; that each room be of ample dimensions; staircases made of iron or stone, and these and all parts of the building to be lighted by gas; that the offices and heating apparatus should be distinct; that the room for the

excited should be on one side of a corridor, well lighted and ventilated, and that the whole should be inclosed with an invisible or unobtrusive fence. These declarations were solemnly reaffirmed in 1871, although, practically, many modifications have been made in their application.

The remarks in these pages advert to all asylums indiscriminately, whether erected by public taxes or private benefactions, whether under the rule of States, corporations, or bodies of trustees. But in order to be explicit we would represent them as consisting of four categories—State asylums; charitable institutions; county hospitals corresponding to our Borough Asylums, but which are chiefly available as reservoirs into which the superabundant insane population of other institutions may be drained; and of almshouses, somewhat but very remotely assimilated to insane wards in British workhouses. All of them, according to Dr. Ranney, have been, or are upon their trial, many have been tried, many condemned, many more persecuted. That there are abuses to be corrected, defects to be repaired, in such establishments, is inevitable, but the inquisitions to which we refer seem to have originated in great part in the natural, perhaps justifiable, jealousy of the citizens of a very free country, of secret if not stern, and of but slightly controlled or responsible authority, in the suspicions of the timid, in the delusions of the half-crazy or wholly deranged; to have depended for support and verification upon testimony, often undisclosed, delivered in the absence of the accused; to have been instituted in many cases by individuals having no legal or judicial status, and to have been conducted in what would here be regarded as an unconstitutional manner, with closed doors, without assessors, and while those affected were ignorant of the accusation. In one inquiry the charge depended chiefly upon the statements of five lunatics.

In general these allegations as to contravention of law, maltreatment, and other corrupt practices, have been refuted, but in one the triumph of the person arraigned inspired him with so much disgust and distrust as to eventuate in his resignation. These proceedings and many other circumstances have suggested the formation of a central commission, who might adjudicate upon such occasions, and deal with all matters connected with the isolation, treatment, and general interests of the insane, somewhat resembling our boards of lunacy. Such bodies have now and then been called spasmodically into existence in America. Their functions have been limited to individual States, have been directed to some special object, and have never been continued longer than a year. While there is an inherent repugnance on the part of our brethren to the creation of a

class of supervisors or inspectors drawn from lay, extraneous, and, it might be, querulous portions of society, and empowered to criticise and perhaps condemn that of which they were profoundly ignorant, we do not conceive that any objection would be offered to the appointment or operation of a board consisting of distinguished members of their own speciality, of lawyers of tried experience and fidelity, and of functionaries of elevated rank connected with the Government.

As an illustration of what even a local and temporary experiment of this description might accomplish in gathering information, we will quote a few items from the report of the Commissioners of the State of Massachusetts for 1874. The insane population amounted to 4000, of whom a large proportion were aliens. The actual increase of insanity is attempted to be demonstrated. While in 1854, 304 acute cases were deposited in almshouses, there were in 1874, 516 there or in private dwellings. Early treatment gives 70 per cent., in late stages 10 per cent., of cures; but entrants generally diseased for upwards of three months. Five sixths of inmates of asylums are paupers of town or country. Lunatics are mingled with pauper children in almshouses. Superintendents, from insufficient staff, are engrossed with building, agricultural, fiscal operations; are opposed to separation of chronic from acute cases, and if subjected to suitable training should receive a most liberal recompense; are not satisfied with present condition of asylums; recommend minimisation of restraint, which amounts to five to ten in each asylum. There may be just grounds for complaints by patients of harsh treatment; but wherever a commission indicated low diet, dark cells, or unjustifiable correctives, these were abandoned. Implied condemnation of indiscriminate correspondence, sanction of letters to trustees and public officers, enjoin visitation of private asylums by public authorities, state that licences were made obligatory in 1864; note the unwillingness of superintendents to discharge or transfer convalescent and quiet inmates, and moralise on protection of those deprived of civil rights; lament lack of classification and of suggestions of developments in annual reports, in which they seem to desire an advocacy of hygiene, of education as a preventive of mental disease, and point to the unfruitfulness of forty reports from a crack asylum. They comment upon the invasion of insanity at an earlier age than formerly, of the predominance of hereditary tendency as a cause. They suppose that anomalous cases are sent by courts of law to jails, asylums, and almshouses. They erroneously conceive that accidents are more numerous in large asylums in Britain than in small, but judiciously urge that they should

be reported and examined into by public officers, and amusingly conclude by endorsing the theory that, besides the achilles induced by agglomeration of lunatics, the walls of hospitals become impregnated, with an animal, and, therefore, destructive poison. This valuable document is followed by an appendix, which, if not flowing from the pen of an English Commissioner, must emanate from a spectator who sees the boons and blessings conferred by the Board of Lunacy through a magnifying, as well as a multiplying, medium.

In 1846 the cloistered lunatics were supposed to reach 3377 in number; in 1860 Dunglison calculates that the gross insane population, including those in seclusion and those at liberty, reached to about 31,000, or 1 lunatic or idiot in 1400 of the inhabitants; but these statistics are, from the fallacies and uncertainty attending the taking of the census, nearly as conjectural as the statement of Dr. Pliny Earle, who in 1867, represents, on the authority of the census of 1860, the Insane as having been ascertained to be 24,000, but assumes that the actual total should be 40,000. To accommodate the exigent cases of these classes there are now 76 establishments, exclusive of almshouses, but their capacity cannot even be guessed at, as a table published in 1862 contained only such hospitals as were then finished, and was, confessedly, imperfect, inasmuch as those supplying the wants of States that had been separated from the Union by civil dissension were excluded. The editor of the '*Journal of Insanity*' has promised a new and exhaustive edition of this important document.

The unfortunate victims to alienation for whose accommodation this vast machinery is intended, are deprived of liberty and their civil rights, not summarily, but by a process of law. This process, unfortunately, differs so widely and irreconcilably in different states as greatly to impede the smooth and successful working of the general machinery, and as greatly to perplex a critic accustomed to the simple and uniform character of the law in his own community. It would appear that in primitive times it was imagined that the insane would be better sheltered and cared for among their friends than among hired strangers or at a distance from home, and accordingly no public provision was made for their reception or treatment, and no legal interference was demanded, or would have been tolerated in what was strictly a domestic arrangement. But, as colonisation proceeded, and as bodies of citizens grouped themselves into distinct communities with independent constitution and jurisdiction, and as scandals and rumours of negligence and oppression circulated abroad, usages grew up almost spontaneously, and enactments were framed in accordance with the spirit and

requirements of the people and the period, and hence were created the discrepancies and confusion which still obtain. Repeated efforts have been made to press upon the Government the expediency of a general law, but, until very lately, it is doubtful whether even those engaged in the execution of the law affecting lunatics in one State had a clear knowledge of what obtained in other members of the Union, and until 1871 no *vidimus* or collation was accessible of the agreement or disagreement among such provisions. In that year Dr. Ray produced his 'Synopsis of the Laws affecting the Confinement of Lunatics in certain States of the Union,' a most valuable contribution, but which does not serve to extricate altogether from the difficulties of the subject. We had attempted to tabulate the terms of admission, detention, guardianship, and discharge of patients throughout the States, but the result was too voluminous for our pages, and we are compelled to confine our abstracts to the most marked and momentous differences and peculiarities in thirty-five states, as compared with that of Maine, where the forms, though complicated, are more complete than elsewhere.

*In Maine.*—Insane minors sent to hospital by parents or guardians. In all other cases commitment by municipal authorities. There is an appeal to justices of peace. Insane prisoners committed by court to asylum, before trial, for observation. Discharged, if cured or inoffensive, by Judge of Probate, &c., on application and recognisances. Ordinary patients discharged after six months by municipal authorities. Guardians appointed by Judge of Probate.

*In New Hampshire.*—One medical certificate required. Dangerous patients committed by Judge of Probate, with intimations to Selectmen, &c. Paupers committed by overseers of the poor, or Court of Common Pleas. Discharges by trustees of asylum, or by justice of superior court, as they conceive expedient.

*In Vermont.*—Private patients confined on one medical certificate, by friends. Dangerous, convict, or criminal lunatics to be confined by court in common jail, hospital, or elsewhere, till cured.

*In Massachusetts.*—Two medical certificates required, with history of case and notice to mayor. Jury may be summoned by judge: or alleged insane prisoners to be examined and certified by physician of jail and four medical superintendents. Lunatics supposed to be unjustly confined to be examined personally by three citizens.

*In Rhode Island.*—Patients confined on one medical certi-

ificate by legal advisers, friends, &c. ; if paupers, by municipal authorities.

*In Connecticut.*—One medical certificate necessary, but three assessors, one a lawyer, may be appointed. Commissions of inquiry may be granted, after six months, on application of respectable citizen or of officials of the asylum. Hospital authorities may discharge.

*In New York.*—Two medical certificates required, but furious persons confined by two justices until expiry of ten days. Jury may be summoned in alleged lunacy. Discharge and destination of criminal lunatics to be regulated by certificate of superintendent. Supreme Court has care of person and property of lunatic, but appoints Committees of person and property.

*In New Jersey.*—Medical certificate required in every case.

*In Delaware.*—Trustee of poor required by Chancellor; &c., to place lunatics in confinement. Discharge by Court of Levy.

*In Maryland.*—Pauper and dangerous lunatics confined by process before jury. Court may send criminal lunatics to almshouse or hospital.

*In Columbia.*—Secretary of Interior, on two medical certificates, may confine lunatic.

*In Ohio.*—Patients confined by complaints on oath, with medical certificate. Criminals sentenced to death may be pardoned by Governor.

*In Indiana.*—Lunacy tried by jury of six. On appeal to Common Pleas by jury of twelve. If sane, expenses fall on complainant.

*In Illinois.*—Lunatic confined by process before jury on two medical certificates.

*In Michigan.*—Probate Judge orders confinement of indigent lunatics on two medical certificates and other evidence. If grand jury reject indictment, lunatic may be either committed or discharged.

*In Wisconsin.*—Medical certifiers must belong to the same county and their respectability attested. All lunatics entitled to habeas corpus.

*In Minnesota.*—Private patients confined by relations on opinion of asylum superintendent, without medical certificate.

*In Iowa.*—Seclusion and all matters connected with insane determined by commission, one member being a physician.

*In Kansas.*—Temporary confinement by relatives, guardians, Probate Judge, who must comply with the deliverance of two consecutive juries.

*In Nevada.*—Lunatic tried before district judge and two



physicians. If curable, &c., handed over to the Secretary of State, who confirms.

*In California.*—Proceedings as in Nevada.

*In Virginia.*—Patients confined by unanimous vote of managers of asylum, or by trial before justice.

*In West Virginia.*—Patients confined by opinion of asylum superintendent and one director, or by Justice, who may, however, deliver accused to friends.

*In Kentucky.*—Jurisdiction over insane vested in Chancery and Circuit Court of respective counties, or any judicial functionaries.

*In Tennessee.*—Indigent lunatic committed by certificate of Justice and Physician. Asylum superintendent discharges if patient sane or otherwise unfit.

*In North Carolina.*—Same proceedings as in Virginia, but one medical witness necessary.

*In South Carolina.*—Incurable and harmless exempted from confinement by justices.

*In Georgia.*—Seclusion and appointment of committee by justices of inferior courts; same proceedings after relapse.

*In Alabama.*—Judge of Probate commits on medical testimony; jury optional.

*In Mississippi.*—Lunatics may be transferred from penitentiary to asylum on advice of physicians. Orphan's Court on application may summon jury and appoint guardian if insanity proved.

*In Missouri.*—Temporary confinement by friends until County Court declare insanity.

*In Arkansas.*—*Vide* Kansas. Jury not obligatory.

*In Louisiana.*—Interdiction granted by judge of parish on personal inquisition and medical certificate. Curators to report to judge every three months.

*In Texas.*—Chief justice of county tries lunatic before jury of twelve men. Confinement on application by relation or guardian dated within two months.

In certain States no legal provision seems to exist.

No legal forms of certificate, &c., are prescribed, but are provided in several states, and express the opinion of the Physician, not the facts on which that opinion is founded.

In order to harmonise or amalgamate these divergent elements in the codes for the protection of the unsound, Dr. Ray has incorporated with his exposition the programme of a general law, now in force in Pennsylvania, for the consideration of the less favoured communities, and which embodies the following principles:

1st. That no insane person can be confined, whoever the

applicant may be, without the attested certificate of two medical men granted within one week.

2nd. Interception of letters by patients to counsel by medical superintendent punishable by fine of 100 dollars.

3rd. Habeas corpus granted by judge on application of respectable citizen.

4th. Criminal lunatic acquitted by jury, to be placed in asylum.

5th. Discharge of such lunatic to depend on evidence before judge if criminal act was the first committed; if second, and that homicidal, &c., to be detained until cure be proved.

6th. A commission, one member being a physician, may on evidence report to judge, who may confine.

7th. Judge may or may not order discharge on representation of loss of bodily or mental health of criminal lunatic.

8th. Discharge effected by responsible parties.

9th. Insufficiently supported insane patients may be placed in asylum by judge, he making responsible parties liable.

10th. Superintendents exempted from all responsibility if legal provisions be complied with.

On application of party interested, commission may be appointed and summon a jury of six or twelve to try a question of lunacy, and if approved to appoint guardians.

It would be unseemly and ungrateful to conclude this epitome of certain aspects of psychology on the other side of the Atlantic without tendering homage and acknowledgment to such honoured alienists as Jarvis, Ray, Kirkbride, Pliny Earle, Curwen, Gray, for the store of knowledge communicated in their works, for their life-long devotion to the progress of science, and for the noble example offered to the members of their speciality in all parts of the world, in their sympathy and exertions in alleviating the sufferings and sorrows of their fellow-men.

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## VI.—Weather and Mortality.<sup>1</sup>

As dwellers in the British islands, so proverbial for their very frequent and uncomfortable variations in meteorological conditions, we are by nature, or, speaking more scientifically, by evolutionary characteristics, most observant of and keenly

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<sup>1</sup> 1. *Journal of the Scottish Meteorological Society*, July, 1874—October, 1875.

a. *The Influence of Weather on Mortality from different Diseases and at different Ages.* (First paper.) By ALEXANDER BUCHAN, Meteorological Secretary, and Dr. ARTHUR MITCHELL, Chairman of the Medico-Climatological Committee.

b. *Report on the Mortality of the large Towns of the British Islands in relation to Weather.* By A. BUCHAN.

alive to the weather. The first, and sometimes the last and only words between taciturn Englishmen, have reference to the weather. The topic is always a safe one to begin with, and usage sanctions its repetition at any hour of the day. It might reasonably have been imagined that the great national interest displayed respecting the weather, and the millions of daily observations made with regard to it, would have ended ere this in the elaboration of a weather-lore, possessed of scientific accuracy and value. Facts, however, tell us to the contrary. To the mass of Englishmen the weather remains a mystery. They know not whence the wind cometh, or whither it goeth; its rise and its direction or course appear arbitrary, bound by no rule. And yet as an agricultural people, and still more as a commercial nation having the largest stake in ships and shipping, Englishmen have the most direct and the strongest interest in the discovery of the laws which regulate the weather, and which direct storms and tempests. Nay, further, their everyday experience demonstrates to them the important part played by the weather on their physical and, in no small degree, on their mental well-being.

Happily, therefore, for us all, a small band of scientific observers have addressed themselves to the solution of the various questions embraced in the philosophy of the weather, or, in a word, in the science of meteorology; and latterly, several of our principal newspapers have sought to bring under popular notice the daily observations and prognostications of weather made at a considerable number of meteorological stations, both within the area of our own country and in some Continental States; and from the systematic observations now made in all parts of the civilised world we may hope to see some definite laws established for the winds, some reliable inferences as to weather, and some practical conclusions as to the relations between meteorological conditions and disease.

To the last-named subject the essay before us is a most important contribution. We know of none equal to it in compass or in painstaking accuracy. It is the result of the labours of two gentlemen, Dr. Arthur Mitchell and Mr. Alexander Buchan, both well versed in meteorology; the former being chairman of the Medico-Climatological Committee, and the latter a secretary of the Scottish Meteorological Society, and both alike contributors of many papers read before that useful and distinguished society. Without having had experience in the same or in similar work, it is impossible for the reader of the paper to appreciate the extent of labour involved in educating and illustrating the facts and inferences placed before him, yet

he cannot fail to be impressed by the mass of information conveyed and by the evident carefulness of the work done.

We shall first describe the writers' own account of the basis of their labours, and of the methods pursued by them in evolving and illustrating results; and, afterwards, we shall place on record some of the more prominent conclusions and offer some critical remarks on the whole matter of the essay.

The materials used in investigating the relations between the weather and disease are the returns of deaths in London for thirty years, and the meteorological data recorded during that same period in that metropolis. London has been selected "because it affords—(1) an enormous population, spread over an area so limited that it may be regarded as having one uniform climate during each of the seasons of the year; (2) full *weekly* reports, giving returns of the deaths from the different diseases; and (3) returns extending over a sufficiently long period. This inquiry embraces the thirty years beginning with 1845 and ending with 1874."

To express the conclusions arrived at, and to make them obvious to the eye, the plan of representing the upward and downward movements of the several diseases, for each month of the year, by curves, is very judiciously adopted. An inspection of the figures employed will best make intelligible the plan; but, suffice it to say that, a straight black horizontal line, in each figure, is drawn to represent the *mean* weekly death-rate on an average of the fifty-two weeks, over the thirty years' period investigated. This line is intersected by black lines which divide it into twelve sections, each of which represents a month; and these sections again indicate by short, finer subdividing lines the fifty-two weeks of the year.

"The average death-rate of each week is compared, and the difference above or below is calculated in percentages of the mean weekly death-rate for the whole year . . . . When the percentage for any week is *plus*, the amount is placed above the black line of the figures representing the mean, and when *minus*, it is placed below it. . . . From these percentages above and below the averages the curves representing the death-rate of the different diseases from week to week have been constructed."

In some instances Bloxam's method of laying down curves has been adopted; for example, where the returns of mortality from a particular disease have extended over only a short term of years, or where deaths from a special malady have been few.

The *causes* of death dealt with are those approved by the Registrar-General, and which alone are available for use under the circumstances of the inquiry.

"Except for those diseases which seem to be directly and immediately under the influence of temperature, the deaths from which rise and fall with the rising and falling temperature, or *vice versa*, such as diarrhœa and bronchitis, the results of this inquiry show that, for any other particular disease, an average weekly death-rate of about twenty for a period of thirty years is required to furnish the data for the construction of a curve of the weekly death-rate smooth enough to be regarded as a satisfactory *constant* of the mortality from that disease as distributed over the weeks of the year. Since a curve of this character has been arrived at in the case of many diseases, it follows that a large number of the curves given in this paper may be regarded as true *constants* for the disease to which they refer; and such *constants*, it need scarcely be added, London alone, with its enormous concentrated population, could have given. In the beginning of the discussion, the great advantages of a thirty years' average over say a twenty, or even twenty-five years' average became apparent, and a thirty years' period was accordingly adopted. That a division of the years into months is insufficient in inquiring into the influence of weather on mortality, and that a division into weeks is essential, are immediately apparent on a simple inspection of the curves; it is evident, for instance, that in the case of the curves for diarrhœa, convulsions, phthisis, and many other diseases, monthly averages would either wholly obliterate their more striking features, or smooth them down to such an extent that they could not be regarded as any longer disclosing the real relations of weather to the progress of mortality from these diseases."

Unfortunately the line of inquiry here pursued cannot be extended, and the relations between the *prevalence* of diseases and the weather exhibited, for the requisite registration of diseases, is wanting. The registered mortality from diseases is the only exponent of their prevalence in the community available. And even this, imperfect as it is of itself, has defects of extrinsic character attaching to it. Thus, the date of registration of death is an after-event, at a more or less interval from that of the onset of the illness causing it; and, again, it is common to allow accumulation of entries of deaths to take place until the periodical official returns are called for.

The reasons, and very sufficient ones they are, for selecting London for the inquiry under notice have been stated; but the selection of a single city, however populous, and however fairly it reflects the operation of the ordinary climatic conditions of the country, cannot accurately indicate the effects of meteorological phenomena in other towns which differ from it, and also from one another, in geographical position and surroundings, in the matter of exposure to certain winds, in geological relations, and in social and sanitary conditions.

In fact, investigations carried out in various towns of the United Kingdom clearly show that the relative mortality of

London at different seasons, both when viewed generally and in connection with particular diseases, does not correctly represent the mortality of the seasons elsewhere. The very next number of the 'Journal of the Scottish Meteorological Society' (July—October, 1875) affords demonstration of this fact, and so bears out the impression Dr. Mitchell and his colleague, Mr. Buchan, had formed. It is the latter able observer, indeed, who reports on the question in the publication just quoted.

"The results [he observes] for every one of the large towns show, during the winter months, an excess above the average mortality." But this excess varies considerably in amount in different cities, and exhibits itself at periods of the winter more or less diverse in various places. For instance, at Dublin the highest monthly mortality is from four to six weeks later than the maximum of English and Scottish towns. In English towns, again, the minimum mortality is in the spring, but in Scottish towns it is in the autumn.

The differences in the rates of mortality of different British towns stand out most prominently in the hottest weeks in the year; but in all towns alike, in England, there is then an excess of deaths above the average. To this general rule Bristol appears an exception, for although the mortality during such period rises, it never quite reaches the average. On the other hand, Leicester has the ill fortune to surpass by a long distance, in summer mortality, every other town in the country.

The above rule of the hot season, obtaining in English cities, does not hold good in Scottish towns, nor, indeed, in Dublin. There is, on the contrary, a reduction of the mortality below the average during the summer months, a circumstance particularly observable in Dublin. Glasgow and Dundee are somewhat exceptional, for they exhibit a rise in the proportion of deaths, although the average rate is not quite reached.

Further, if we examine the details before us with reference to particular diseases, we likewise find great variations subsisting between the several towns in the rate of mortality from those diseases. And the conclusion is inevitable, that many circumstances due to topography, social conditions, sanitary states and occupations, very materially modify and disturb the working of simple meteorological states. In fact, Mr. Buchan himself remarks of the outcome of his observations on the varying death-rate of diarrhoea in different towns—"This points to other causes than mere weather, particularly the relative temperature and humidity of the place, as determining the absolute mortality" (p. 339). Furthermore, it is worth while to note that the relative proportion of infant lives to adult lives,

and of female to male lives, will materially affect the curves indicating the effects of weather upon mortality. To realise these facts the reader has only to turn to the section on the "influence of weather on deaths at different ages," and particularly in respect to diarrhoea and pneumonia. Nevertheless, the research which has been completed for London furnishes a great and accurate groundwork for comparisons, and will be useful at all points in further investigations regarding other places.

In fact, having this excellent basis thus supplied for London, we may fairly anticipate that inquiries, similarly carried out in other large cities, will furnish such materials for comparison as may help to solve some of the mystery attaching to the prevalence of many sporadic, and probably of some endemic maladies.

The order in which our authors deal with their subject is—1st, the distribution of deaths from different diseases over the weeks of the year, each disease or cause of death enumerated in the nosology of the Registrar-General being taken in turn; 2nd, remarks on groups of curves between which there is a coincidence, or otherwise a reverse order of results; 3rd, in connection with the foregoing the relations of weather to deaths from different diseases without regard to age, and also, as far as practicable, with respect to age; and 4th, those same relations with reference to the sexes; but here, unfortunately, the returns of the Registrar-General fail to furnish the necessary materials, and nothing more can be effected than to show that the two sexes are not always affected by weather, either in an identical manner or in the same degree. The paper concludes by a series of tables containing the calculations from which the curves have been constructed.

The reader is very properly reminded that the object of the inquiry is not to show "what diseases are most fatal, but the manner in which the progress of different diseases is influenced by weather. A disease which kills comparatively few may show itself to be in a remarkable manner under seasonal control; whilst another disease, which kills a great many, may have a steady progress, varying little from week to week. It is with this variation we deal, for the purpose solely of determining whether it appears to be related to and influenced by weather." For instance, phthisis is a disease which exhibits small sensitiveness in its rate of mortality to weather, for its elevation above the average in no week exceeds 13 per cent.; whilst, on the other hand, asthma, which is chargeable with vastly fewer deaths, has a range of 126 above and 75 below its average, clearly determinable by the state of the weather.

It is not possible to reproduce the results of the inquiry as presented to us for each officially recognised cause of death, and we must be content with passing remarks on the conclusions exhibited in regard to some of the most important assigned causes, and to some groups of such which seem to point to a common pathological likeness in their meteorological conditions of mortality.

The first diagram given represents the curve of "deaths of both sexes at all ages and from all causes, for thirty years." Its chief features are—a high and prolonged maximum, extending from the middle of November to the beginning of April; and a high but short-continued secondary maximum from the middle of July to the middle of September. The absolute minimum of the year occurs in the middle of June, and the secondary minimum in the second week of October. The most rapid upward flight of the death-rate curve occurs between the second and the fourth week in July; but a continuous rise goes on from the second week of October to the second week in December. There is a rapid but steady fall from the beginning of April to the middle of June, and nearly as rapid, but an interrupted one, extending from the middle of August to the second week in October. The absolute maximum elevation is attained in the second week of January.

An examination of the curves of the several diseases which are presented in order shows that none of them resemble the general curve for all diseases, just referred to. It likewise shows, as might *à priori* be surmised, that the maximum of the winter months, extending from the end of November until the end of March, is attributable to diseases of the respiratory organs pre-eminently; and, further, that the rapid elevation at the end of July, prolonged into August and September, is pre-eminently due to bowel complaints. These facts are well brought out by a subsequent diagram (fig. 61, p. 237).

As a matter of course there are many contributory diseases concerned in the building up of the maximum representing each of those two great groups of diseases. So, on the other hand, there are many maladies which have a curve of an inverse order, and which consequently, in any diagram representing the mortality from all causes, tend to neutralise or lessen both the extent and rapidity of the upward and downward movements delineated.

Among those maladies which concur more or less closely in seasonal mortality with diseases of the respiratory organs, are—measles, erysipelas, continued and puerperal fever, rheumatism, privation, mortification, apoplexy, paralysis and brain diseases, pericarditis, aneurism, childbirth and old age. So, again, the



curves of the following accord generally with that of bowel disorders—want of breast-milk, thrush, *tahes mesenterica*, enteritis, jaundice, atrophy, and debility.

But when we look to fig. 61, already quoted, we cannot help remarking on the *comparatively* small effect of weather on the mortality *at large* in regard to the whole category of assigned causes, after that deaths from respiratory diseases and bowel complaints have been eliminated, so enormously do these two groups of disease affect the curves. Nevertheless, an inspection of the diagrams given for the several diseases puts beyond doubt the potent effects of weather in regard to a majority of them, and likewise makes evident that those effects are very unequally felt. Yet it remains as a general fact that, excluding diseases of an inflammatory character, and particularly those of the respiratory apparatus, and disorders of the alimentary canal—in the form of diarrhoea, dysentery, and cholera,—heat and cold, winter and summer, do not exercise so decided an influence on mortality so far as concerns other maladies. The curves of many, indeed, intimate other agencies to be at work; but thus much they reveal, that such diseases observe a sort of periodicity in their fatal issue.

The so-called zymotic diseases exhibit much diversity among themselves in their mortality curves, although cold appears as an aggravating cause with almost all of them. The maximum of smallpox is found to be during the first seven weeks of the year; that of croup extends from the middle of November to the end of April; and that of quinsy, of erysipelas, and of puerperal fever, from October to March. "The measles curve is remarkable in showing a double maximum; the larger occurring in November, December, and January, and the smaller in May and June; the larger minimum in August, September, and October, and the smaller in February and March."

Scarlatina displays an indifference to the simple conditions of heat and cold; and the same may be said of typhoid fever, the curves of these two maladies corresponding in general features, and showing a maximum elevation in October and November. And it is well deserving of notice at the present time, when the question of identity between the two maladies is so warmly debated, that the curves of croup and diphtheria do not precisely accord. The curve of the former remains above the average till the end of April. Its minimum period is also more pronounced. However, the value of any inference deduced from these variations, which are not very serious, will be variously judged according to the amount of credit given to the official returns of deaths and the carefulness exercised in diagnosis. Moreover, it has to be kept in mind that the cases

of diphtheria were not separated in the returns from those of scarlatina until 1859, or only over a space of sixteen years of the thirty analysed

The mortality curve of hooping-cough offers a contrast to that of scarlatina. It does not at any time mount up so rapidly; it attains its maximum in the spring, when that of scarlatina is at its minimum, and its minimum in the autumn, when the latter is at its maximum. In short, it agrees with the curves of nervous disorders and convulsions.

"Previously to 1869 all fevers were classed together, but from the beginning of that year they were separated into typhus, typhoid, and simple continued fever." A diagram is given of the seasonal mortality of fevers, as undistinguished from each other, and we are next presented with a figure for typhus, typhoid, and continued fever, severally and particularly. The general conclusion arrived at by Dr. Mitchell and Mr. Buchan therefrom is, that "typhoid fever is the first to rise above its average, in the middle of August; simple continued fever next, in the end of November; and typhus last, in the end of December. On the other hand, typhoid falls to its average in the beginning of January, simple continued fever in the end of March, but typhus not till the second week in May. It is in consequence of the marked differences in the course of these fevers over the weeks of the year that the general curve for all fevers lies so close to the mean line, a maximum period of one frequently occurring at the same time as a minimum period of another."

Without calling in question the correctness of these conclusions flowing from the premises at the writers' command, we hesitate to accept them. This hesitation arises from the following considerations. In the first place, the short period, six years, for which the averages have been struck, necessitating the employment of Bloxam's method; and in the next place, and for the weightier considerations, the loose manner in which the nomenclature of fevers is employed in the returns of deaths, and failure or neglect in differentiating between the several fevers. For every one conversant with registration returns knows that a typhoid state is sometimes set down as typhoid fever, and that in the list of causes of deaths are the anomalous terms of brain fever, gastric fever, throat fever, remittent fever, &c.; and that, on the other hand, the term 'continued fever' is so elastic that it covers any form of fever, and embraces cases of typhoid as well as typhus, particularly of the former. And our conviction is, that we must await the lapse of another epoch of thirty years, an improved state of official returns of deaths, and the general recognition of the pathological differ-

ences between typhus and typhoid fevers, and also relapsing fever; differences which, though so admirably portrayed several years since by Sir William Jenner, have by no means been as yet universally recognised, or, indeed, generally regarded as established.

There is a close agreement shown between erysipelas and puerperal fever as regards the influence of weather on their mortality; but, both in its maximum and minimum range, the curve of puerperal fever is much more decided. There is a rapid rise in September, which proceeds to the highest point (50 per cent. above the average) at the close of November, and after declining to the end of December, commences again to mount to a secondary but lower maximum in January. The depression below the average is very marked in July and August, and until the middle of September.

The popular notion that apoplexy and paralysis are more common in cold weather than in warm is borne out by the diagrams produced.

"The curve for paralysis follows more closely than any other curve, with the exception of the curve for old age, a course inversely as the temperature, being below the average from the beginning of May to the middle of November, and above the average during the rest of the year. It differs from the apoplexy curve in having its maximum fatality in the middle of winter, and in remaining more steadily at some distance below its average during the warmer months of the year."

Considering the pathological relations subsisting between these two registered causes of death, so that one cause may in many instances be readily substituted for the other, and that both alike, for the most part, arise from similar nervous lesions, and that one expresses an after-effect of the lesion which produces the other (excepting, indeed, so far as fatal paralysis is due to spinal mischief and to the effects of poisons), the attempt to appreciate and portray the different effects of weather on their mortality as distinct lesions can realise no very substantial fact. Many a death from heart disease gets entered as a consequence of apoplexy, and in the case of numerous deaths attributed to paralysis the real cause is not paralysis but some intercurrent malady.

Before quitting this portion of the essay we would note that gout has a distinct, constant, and special curve, showing its mortality to be in excess in March, April and May, and slightly so in January; and that jaundice exhibits a decided maximum in August and September, and reaches its minimum during the winter months, except about the new year, when the festivity of the season has probably much to do with its upward move-

ment. In many respects its curve resembles that of hepatitis, but it presents double the summer excess the latter does, and, consequently, in respect of this rise in summer, exhibits an affinity with bowel complaints. Mortification bears a considerable resemblance in its seasonal mortality to nervous disorders; whilst old age repeats by its curve that of respiratory diseases, showing that it usually terminates by the intercurrent of those maladies.

In a concluding paragraph the authors indicate a fruitful inference respecting the epidemics and the aspects they may assume according to the season in which they prevail.

"Sometimes [they remark] in diseases which are epidemic, one invasion is characterised by the frequent occurrence of pulmonary, another by the frequent occurrence of nervous complications. It is possible that the season of the year at which the epidemic occurs may determine this. In other words, a certain character may be given to an epidemic by the season of the year at which it presents itself."

And in illustration of this inference they appeal to the statement of Dr. Theophilus Thompson in his 'Annals of Influenza,' "that in the epidemics occurring in the spring, as, for example, in the years 1658, 1743, 1762, 1782, and 1803, the head and nervous system—and in those of autumn, as in 1775 and 1836, the bowels—were most affected. This [they add] is in accordance with what this research would lead us to expect" (p. 243).

We may now advance another step and examine the effects of weather on deaths at different ages, keeping before us the results arrived at for all ages. The mortality at different ages, for both sexes and all causes, is illustrated by a diagram (fig. 57) on p. 228. A glance at this at once exhibits the marvellous influence of the seasons on the mortality of children. After eliminating children under five, we have brought to view a series of similar curves, for all ages from five to eighty, which rise rather sharply from the beginning or middle of October to a maximum elevation in December, whence they decline in January, and after making an interrupted upward movement to the end of February, again subside progressively till their lowest point is reached in September. The line representing the seasonal mortality of children between one and five closely resembles in its contour those spoken of, but it also expresses a high mortality in that interval, and a marked influence of both hot and cold weather in inducing it.

But the line exhibiting the mortality of infants under one, whilst running parallel with the rest from January to the middle of June, and from the third week in September to the close of the year, makes a sudden and most formidable upward

movement, culminating at the end of July in a point not so very far below that indicative of the absolute maximum elevation attained in December.

The interpretation of this is at once arrived at by reference to former figures, even without the telling one (fig. 58) on p. 231. The excessive mortality, in summer, of infants under one year of age has for its almost sole factor the prevalence of diarrhœa.

To quote the essay before us—

“Of the 69 deaths (from diarrhœa) which occur weekly, no fewer than 62 are children under five years of age, thus leaving only 7 for the rest of the population taken together. The proportion of deaths among children is much smaller in those weeks when the average mortality is small. Thus, in April, when the average weekly mortality is 14, only 9½ occur among children below five years of age, or only about double the mortality at all other ages. But during the warmest weeks of the year, when the mortality from diarrhœa rises to a very high figure, the increase of deaths caused by it among persons under five years becomes not only in itself enormous, but it proceeds at a strikingly accelerating rate with the increase of temperature; so that during the three weeks of highest mortality, when the weekly average for the whole population reaches 348, no fewer than 327 of these are found to be children under five years of age—in other words, during these three weeks the mortality from diarrhœa among children under five is sixteen times the mortality for the same disease among persons at all other ages taken together. . . . [Further] the vast increase to the death-rate from diarrhœa during the few hottest weeks of the year is almost wholly drawn from infants under one year of age, . . . and occurs unfailingly from year to year; [whence it follows that] the summer maximum of mortality in the general population is almost wholly due to the fatality of diarrhœa among mere infants.”

We append two or three more illustrations of seasonal mortality in connection with age. As regards phthisis, the mortality of young persons falls to the annual minimum at the approach of winter, whilst that of individuals between forty and sixty rises above its average at the same season. “It is from about fifteen years of age that the mortality from phthisis begins to increase largely, and at the same time it begins to show a sensitiveness to weather influences.

In the case of bronchitis, which is most fatal to the very young and the very old, the great annual rise in its mortality takes place between the middle of October and the end of the year; but in the case of those above twenty years old the deaths do not rise till three weeks later than they do with the young, a fact indicative of the greater sensitiveness of the young to the effects of cold than those of mature years. Again, pneu-

monia attains its maximum mortality in November and December, but this maximum is principally made up of the deaths of children under five, and to this same cause the character of the curve at this season is chiefly due. "The two extremes—viz. the very young and the very old—resemble each other in having the annual maximum about the end of November; at other ages it occurs from three to four weeks later. The March secondary maximum is well marked at all ages."

One more instructive illustration of the subject under notice will suffice. It is afforded by the diagram exhibiting the effects of weather on the mortality caused by atrophy and debility among children and old people respectively. In the previous division of their subject the authors produce a diagram for atrophy and debility at all ages, from which we learn that the mortality from these assigned causes begins to rise in June, and reaches its maximum at the beginning of August, from which date it falls steadily to the beginning of October, and after that period continues below the average until the end of June. Comparison of this curve with others shows its agreement with those characteristic of bowel disorder. From the knowledge already gained respecting the relations of child life and the excessive mortality from diarrhœa in summer, we are therefore quite prepared for the fact brought distinctly into view by the figure showing the comparative mortality of the young and old, that the curve for atrophy and debility attains among the former an extremely high maximum in July, August and September. On the other hand, the curve for people above sixty presents a very decided maximum in January and February. Thus it happens that the curves for children and for old persons are of an opposite character; that of the former being, as before intimated, the curve of bowel complaints, and that of the latter the respiratory organs' curve. Moreover, the abstraction of all other lives than those of children under five is followed by an elevation of 70 per cent. above the average in August, as compared with the elevation of the same period of 40 per cent. noted in the diagram for atrophy and debility for all ages. This result again accords with that following the like elimination of all persons except young children from the calculations made in reference to the seasonal mortality of diarrhœa.

As we noted at the commencement of this review, the returns of the Registrar-General are deficient in data to allow the influence of the weather on the mortality of the two sexes to be satisfactorily examined. The only classification according to sex published in the weekly returns are the *deaths from all causes*. In the hands of Dr. Mitchell and Mr. Buchan these general returns are carefully turned to account, and the con-

clusions deduced "that the influence of cold weather is to increase the death-rate of females to a much greater extent than that of males from the middle of November to about the end of March; and that, on the other hand, males die at a much more rapid rate than females during the spring months. During the summer months the death-rates of the two sexes closely approximate to each other." The period of the year more fatal to females than males is that of the highest mortality from diseases of the respiratory organs; whilst that of less fatality to them is when diseases of the nervous system are most destructive. Other deductions made are—that at every age a much larger number of deaths occur among females from hooping-cough; and that, if ages above sixty-five be excepted, the deaths from brain diseases are more numerous among males than females.

To render their essay more complete in its meteorological aspects, Dr. Mitchell and Mr. Buchan have examined the reports for thirty years of the meteorology of London, and have presented a very clear diagram showing mean temperature, dew-point, humidity, and rainfall of the great metropolis. Moreover, assuming climate to be mainly determined by temperature and moisture, including the rainfall, they have divided the year into six periods, each having a climate peculiar to itself, although one shades into another by nice gradations. Taking each period in turn, the authors arrange the several assigned causes of death in six columns, according as they exhibit (1) an increase from the annual minimum; (2) a rapid increase; (3) a large excess above average; (4) a decrease from maximum; (5) a rapid decrease; or, (6) a large deficiency from the average mortality. To a considerable extent these tables represent in a different fashion the results illustrated by the diagrams and their curves, but they bring out more prominently the relations of climatic conditions to the mortality of the several maladies.

We cannot be detained by an analysis of these tables, but must proceed with some general comments upon the whole subject under review.

The value of the preceding researches respecting the influence of weather on the mortality from various diseases resides especially in the length of time over which they extend. The period of thirty years nearly represents a generation of the human race; and, on the assumption that the meteorological changes of this globe revolve in cycles, repeating one another more or less closely, this extended period may be reckoned to include several such cycles. Observations carried out for a few years only would be seriously affected in value, as affording

criteria for determining the effects of weather, by reason of the great variations in the seasons occurring from year to year and the fickle climate we have often to deplore in these islands. But it would, at the same time, be instructive to observe, from year to year, the fluctuations of the mortality from the principal causes of death, and to have the corresponding meteorological conditions precisely noted in connection therewith. And it appears to us that efforts should be made to secure a more thorough and minute registration of telluric phenomena than Dr. Mitchell and Mr. Buchan have employed—doubtless from the absence of material—in their essay. They have kept in view only the three leading atmospheric conditions, heat, cold, humidity, but we are all sensible how greatly these are modified in their effects on the animal economy by the direction and force of the wind, and, in a lesser degree, by the electrical state of the atmosphere, and possibly by varying magnetic conditions. Moreover, it is readily demonstrable how largely those primary meteorological conditions are affected, augmented, diminished, or neutralised by local circumstances, as, for instance, by topographical position and exposure, by elevation above the sea-level, and by the nature of the soil, the quality of the drinking water, &c.

We have already, in a previous page, remarked on the part played by these modifying influences, and have shown that the maximum and minimum mortality differ in the time of year for diseases generally, as well as for particular diseases in different towns.

The curves traced with so much care in the essay before us are true, therefore, only for London. To establish even as general truths the conclusions they point to, we need a parallel line of research carried out in several large towns in the kingdom, and the determination of the curves as possessing similar, if not identical, characters. Nay, further, had we arrived at the determination of “mean” curves of mortality at different seasons for our own country, it would be a bold venture to affirm their accuracy for other lands having different latitudes and a different physical geography. And this reflection suggests the desirability for extended comparative investigations in different portions of the globe.

A careful examination of the “curves” placed before us, and of the accompanying remarks, awakens the idea, which will also arise from a general acquaintance with pathology, that the atmospheric states, heat, cold, and moisture, will not account for many of the facts they shadow forth. In the list of diseases which figure among the Registrar-General’s causes of death are no small number with which, it may safely be



predicated, meteorological states have little or no concern. Although ardently seeking a curve for every officially recognised cause of death, Dr. Mitchell and Mr. Buchan have to give up many in despair, and to admit that no connection with weather can be traced therewith. In the case of not a few others the mortality has been too small to afford the necessary elements for the construction of a curve; and, in most such, we entertain the impression that weather has no relation with their death-rate. Still again, we find difficulty in accepting several published curves as expressive of any actual influence of the weather on the diseases they are presumed to illustrate; a difficulty we must own chiefly attributable to the official nomenclature. Among such doubtful curves we would single out those for dropsy, cephalitis, hepatitis, premature birth, and childbirth, and insanity. We cannot tell what lesions are indicated under the term dropsy; but there is this much may be said of its curve, that it bears a general resemblance to the curves of heart disease and of Bright's disease. So likewise we cannot be sure what lesions are associated under the appellation cephalitis, although it is conceivable that cases of sunstroke may produce the elevation in July, a feature, by the way, that does not appear in the curve for the eminently *general* cause "brain diseases." Again, with reference to hepatitis, if this lesion in a warm climate might justly show an immediate relation to weather, we cannot bring ourselves to the belief that, in this country, it will display a definite curve, such as represented, for we are persuaded that simple inflammation of the liver contributes too few deaths to furnish reliable data for the construction of a curve. Lastly, to detain ourselves with no further examples of the subject, we have a considerable number of deaths assigned to insanity and a curve diligently wrought out; but we would ask, what can it teach? and what is meant by death from insanity? It would be about equally logical and sane to refer a proportion of deaths to "sanity." The psychical derangement does not kill its sufferers, but some physical suffering or lesion, as exhaustion, pneumonia, phthisis, or paralysis; indeed, the running upwards in June of the curve exhibited suggests that a certain number of the insane whose deaths have been reported as due to insanity—for only a fraction of the deaths among insane patients will be so returned—have died from diarrhœa.

Another occasional disturbing element in working out results from the official returns is, change of views among medical men as to nosology. Dr. Mitchell and Mr. Buchan point out this in connection with asthma, to which in former years a very large number of deaths were referred, but of late years a greatly

diminished proportion; bronchitis, on the other hand, presenting an equivalent increase.

For defects such as intimated the nomenclature employed for the registration of deaths is, as just above remarked, chiefly responsible. It has no solid pathological basis; it admits of the registration of the cause of death on the basis of etiology, of symptomatology, of morbid anatomy, and of mere localization, or by the use of general terms expressive of morbid association. Thus, we have numerous deaths set down to atrophy, debility, and want of breast-milk; to asthma and dropsy; to laryngitis, pneumonia, cancer, and hernia; to brain, liver, kidney, uterine or joint disease; and to those widely embracing conditions, privation, premature birth, and childbirth.

But it is deserving special notice, that the results worked out in the valuable paper reviewed contribute to rectify the returns of mortality, and to suggest the true final cause of death concealed under the different appellations employed. This important result follows on instituting comparisons between the various curves figured for the several diseases, and it is one that has not escaped the attention of the writers. When making a special examination of the curves found for diarrhœa, cholera, and dysentery, they remark that they do so—

“In order to point out that deaths from other diseases of the digestive organs have curves which closely resemble those of diarrhœa and dysentery, and may be regarded as substantially the same. In other words, they are influenced by weather in the same way, though the degree of the influence may not be so great. Probably this is true of deaths from all such diseases. It is certainly true that there is no marked exception. It is true, for instance, of deaths from jaundice, and still more strikingly true of deaths from *tabes mesenterica* and *enteritis*.”

In the curves of these maladies no tendency to a winter maximum, such as is uniformly shown by deaths from diseases of the respiratory organs, is noticeable; whilst, on the other hand, they all coincide in having their maximum in July and August, like diarrhœa and dysentery.

Again,

“Deaths from certain causes, not classed as diseases of the digestive organs, also show the diarrhœa curve; for instance, as deaths from thrush, atrophy and debility, and want of breast-milk. . . . The form taken by their curves would indicate that, among those whose deaths are attributed to these three causes, grave bowel complaints are of frequent occurrence. It will be seen that this points to a new use of these death-curves, since it is possible that they may indicate those organs the disorder of which, in the different assigned causes of death, leads generally to the fatal issue” (p. 241).

We are quite ready to grant that the ultimate cause of death in the mortality assigned to the morbid states referred to is, in most instances, diarrhœa ; but we must remark, that the loose terms, thrush, atrophy and debility, and want of breast-milk, convey no actual information, in the majority of cases, of the real pathological changes, or the lesions which are at the bottom of the symptoms designated under those names. They are, indeed, mostly nothing else than terms whereby ignorance of the true morbid state leading to death is hidden. And the same may be said of diarrhœa in regard to a host of the deaths returned as due thereto. But, whatever be the amount of truth in this criticism, it is a clear gain to have the fact made patent by these curves, that the fatal issue is brought about by one common agency, diarrhœa, in several morbid conditions bearing distinct names, which are supposed to represent the true cause of death.

Illustration of the like deductions from the study of the curves may be found by contrasting those for hydrocephalus, teething, convulsions, brain disease, and, though with some limitation, hooping-cough. The examination rather enhances than lessens the value of the diagrams as exponents of the relations of weather to deaths from those assigned causes ; but whilst pointing to a greater fatality at one season than another, and to a possible morbid unity among them, it may be taken to show that what one individual will certify as a death from hydrocephalus another will return as one from convulsions, or teething, or brain disease, or hooping-cough, accompanied by convulsions. It is here worthy of remark, in connection with the teachings deducible from these researches, that hooping-cough ranges itself in the series of nervous maladies, and not, as might be presumed, with diseases of the respiratory organs.

Suffocation ranks legitimately as a cause of death, but no relation would suggest itself as subsisting between it and the weather, yet it is made clear that it is commonest in the cold season ; and we can find a rational explanation of this in the larger use of fire and gas, in the longer nights, and the prevailing drunkenness of Christmas and the New Year, to which cause the newspapers inform us the deaths of many children are attributable, the children being overlaid by their drunken parents.

We have previously observed that the researches now before us demonstrate, what a practical acquaintance with disease will confirm, that meteorological conditions are chargeable only partially with the production and the termination of disease. They may either concur with or be opposed to other active influences. The same exposure to cold may in one individual develop rheumatism, in another pneumonia or bronchitis, in

another quinsy. The like variety of results follows exposure to heat to a less degree.

Further, the conclusions arrived at by Dr. Mitchell and Mr. Buchan, with regard to zymotic diseases, bespeak a greater or less indifference on the part of those maladies to mere meteorological conditions, and the existence of some agency, probably special to each zymotic. We are ready to admit that the effects of cold weather are more or less distinct in most such maladies; for it is in accord with universal experience that their severity and fatality are aggravated by cold, particularly such of them as expend much of their morbid energy upon the respiratory passages, as for instance measles. Yet, taking this example, we notice that if its curve presents a maximum in December and January, it also shows a secondary maximum at Midsummer. Again, the curve of scarlatina clearly proves that its fatality is regulated by some conditions other than what the thermometer can indicate. Its climax is reached in October and November; it sinks to its average in January, and remains considerably below it until August.

Without seeking further illustrations from among zymotic diseases, we may find another in rheumatism. This disease has its maximum mortality in November and December, and a range above the average until the end of April. Yet though this curve apparently shows it to be a disease of cold weather, we feel compelled to look for other causes of its prevalence than cold, or cold and wet combined. In countries where the cold is more intense but drier than in this country it is less common; and on the other hand, in the hot and dry land of Egypt it is a very frequent and fatal disease.

A strong, probably the strongest inference arrived at in the essay before us is, the apparently direct relation between heat and the mortality from diarrhœa, dysentery, and cholera. Yet even with respect to this phenomenon a wider experience and more intimate inquiries into the production of disease will prove that heat is not the sole morbid agent. If heat occupy the foreground and arrest the attention, there is something in the background not to be overlooked or despised.

The mortality curve does vary in "bowel complaints," not only in its range, but also in respect of the months at which it reaches its maximum and minimum. In the number of the 'Scottish Meteorological Society,' July to October, 1875, previously quoted, Mr. Buchan remarks—

"The differences in the rates of mortality from diarrhœa in the different towns are very great, and a comparison of the two extremes, Leicester and Edinburgh, is startling; the figures showing that for every one who dies from diarrhœa in Edinburgh during the summer

months eight die in Leicester from the same disease, in proportion to the population. From the beginning of November to the summer solstice the mortality from diarrhœa is everywhere small, being double, however, in Liverpool and Manchester as compared with London and Portsmouth."

And again, after insisting upon the fact that towns having a lower summer temperature have also a lower ratio of deaths from diarrhœa, and that there is a rise in the ratio in proportion to the increase of temperature, he is fain to recognise the farther fact that "the rate of increase differs very greatly in different towns," and to perceive that—

"This points to other causes than mere weather, particularly the relative temperature and humidity of the place, as determining the absolute mortality. Thus, the summer temperature of Dundee and Perth is nearly the same, and that of Glasgow and Edinburgh is also nearly alike, the excess being rather in favour of Perth and Edinburgh, and yet the diarrhœa mortality of these two towns is respectively less than that of Dundee and Glasgow. It may therefore be assumed that there is something in the topographical, social, or sanitary conditions of Dundee and Glasgow which intensifies the evil effects of hot weather on the health of the people, so as to swell, for instance, the death-rate from diarrhœa at Dundee to double of that of Perth."

The extraordinary mortality from the same disease at Leicester compels the same inference; and, in aiding us to apprehend its cause, we have available the admirable special report on diarrhœa in Leicester by Messrs. Buck and Franklin.<sup>1</sup> These gentlemen look upon the disease in that town as having a "specific" character and to be due to local causes, and that such causes are to be found in organic emanations. The emanations are developed specially by the agency of heat acting upon a subsoil saturated with sewage, or "water-logged." The greater or less prevalence of the malady in the several parts of the town stands in direct relation with the degree in which such a contaminated or saturated subsoil exists. Neither density of population, nor the construction of the houses, nor unfavorable social conditions, exhibit any direct influence on its production.

Further, when an epidemic prevails the apparent immediate relation between heat and diarrhœa and cholera is very considerably disturbed. The annals of cholera tell us of its ravages in Russia even during the severe winters of that empire; and just lately we have had placed before us<sup>2</sup> some very elaborate inquiries respecting cholera epidemics in the United

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<sup>1</sup> Report by W. Elgar Buck and George Cooper Franklin, on the 'Epidemic Diarrhœa in Leicester of 1875.' Presented to the Sanitary Committee.

<sup>2</sup> 'The Cholera Epidemic of 1873 in the United States.' Washington, 1875.

States, which show that in New Orleans, in 1873, the maximum mortality was in May, and the next highest range in April; that in March and June it was about the same and not quite one fifth of that in April; that in July it had nearly reached its end, and that only single deaths occurred in August and afterwards. In other States of the Union the intensity of the disease varied as to the months in which it happened; but it seems to come out clearly that epidemic cholera will pursue its course heedless of seasons, of heat or of cold, though possibly favoured by heat.

And when a cholera epidemic prevails then also do we find diarrhœa to epidemically prevail, and, like cholera, to show itself in places irrespective of its usual concomitant heat. For example, the same American report informs us that diarrhœa prevailed, not only within the area of the cholera epidemic, but at places even remote from it, and that in some such, as in Texas, its destructiveness took place in the middle of winter.

Here we must conclude this lengthened analysis of this essay on weather and mortality, thanking its authors, Dr. Mitchell and Mr. Buchan, for the amount of information they have presented as the fruit of most painstaking, honest, and laborious researches. The essay is not only instructive, but also highly suggestive; and what now is wanting is the carrying out of a course of similar investigations for other large cities, both at home and abroad; the like general plan and the same system of observation being employed, and uniform meteorological investigations diligently made and recorded. J. T. ARLIDGE.

## VII.—Recent Text-books on Obstetrics.<sup>1</sup>

SINCE the publication of Tyler Smith's 'Manual,' a book which marked an epoch in obstetric science, no text-book had, until quite recently, appeared in the English language.

<sup>1</sup> 1. *A System of Midwifery, including the Diseases of Pregnancy and the Puerperal State.* By WILLIAM LEISHMAN, M.D., Regius Professor of Midwifery in the University of Glasgow, &c. Second edition, pp. 848. Glasgow, 1876.

2. *A Treatise on the Science and Practice of Midwifery.* By W. S. PLAYFAIR, M.D., F.R.C.S., Professor of Obstetric Medicine in King's College, &c. In two volumes, pp. 782. London, 1876.

3. *Lectures on Obstetric Operations, including the Treatment of Hemorrhage, and forming a Guide to the Management of Difficult Labour.* By ROBERT BARNES, M.D. Lond., Obstetric Physician to St. George's Hospital, &c. Third edition, pp. 606. London, 1876.

4. *The Student's Guide to the Practice of Midwifery.* By D. LLOYD ROBERTS, M.D., M.R.C.P. Lond., Physician to St. Mary's Hospital (Manchester). Pp. 299. London, 1876.

5. *A Manual of Midwifery.* By ALFRED MEADOWS, M.D., F.R.C.P. Third edition, revised, pp. 496. London, 1876.

Schroeder's book, indeed, has been translated, but it has met with but scant success in its English garb. It seems above or below or beside the requirements of our students and practitioners. It has, however, unquestionable merits. It contains a vast amount of information which it was very desirable to import into our literature. One characteristic it possesses which we should have been glad to see in Leishman and Playfair. It is lavish in its bibliographical references, thus furnishing the reader with the information necessary to correct and extend the knowledge supplied in the text. But, after all, it does not satisfy the British mind, always craving for the practical, and it certainly left a blank for any man who could see and understand what was wanted to fill up. For many years Tyler Smith's book had been out of print; a copy could hardly be borrowed; the author could not be prevailed upon to work at a new edition; and thus a book which might have served as the basis of the best text-book ever seen has fallen out of use in the schools. Its chief defect lay in the description of the operations. Like most men whose field of practice was the West End of London, his experience in difficult midwifery was comparatively limited. He never enjoyed—if it be enjoyment—the ceaseless opportunities of putting to the test, and thus of correcting and perfecting any method of operating that his own genius or the hints of others might devise, such as the teeming East supplies. It is in this respect that Ramsbotham's book still holds a marked superiority. Trained in many a conflict, assiduous practice in coping with difficulties had developed in him exceptional skill, and taught him to discard the absurd theoretical rules, diagnostic and operative, which, sanctioned by Denman, still linger amongst the laggards of professional progress; and which still, we are sorry to find, assert undue empire over the authors of the text-books it is now our duty to examine.

The text-books before us are of two kinds. First we have Leishman and Playfair, text-books of the best kind—that is, works in which the theoretical ground-work is fairly laid down as the basis of the superstructure of practice; in which the reader's mind is properly prepared by fundamental instruction to take an intelligent interest in the subject, and to understand the rules of treatment. Then we have the little books of Meadows and Roberts, books not of the best kind, but which, good or bad in design and execution, seem inevitable. Some minds, we fear many, incapable of industry or of sustained thought, seeking for short cuts by coach, steeple-chase, or otherwise, past the examining-boards, think they find what they want in compendiums which they vainly imagine give

the concentrated essence, the elaborated results, of the teaching of generations. Little books of this kind hardly deserve serious criticism. All original facts and reasoning sublimed away, nothing but a *caput mortuum* of dogmatism remains. No rational being ought to be satisfied with this. It loads the memory, it does not improve the understanding or impart knowledge. The appeal is solely to dull faith. However, such books are inevitable. Reason in many people is tardily developed, or never. We shall not, then, examine these productions in detail, but content ourselves with pointing out some defects which it is the more necessary to do because the young men who cram from works of this kind are little likely to mend the errors they will imbibe by studying works of a more classical character. It is a signal proof of Roberts' ability that although it is scarcely half the size of Meadows' it is the better book.

We will pass on to the more useful and legitimate works of Leishman and Playfair. Barnes' work, now in its third edition, is not strictly a text-book. It deals simply with difficult midwifery. Its stamp is essentially original. It is the expression of large experiences like Ramsbotham's. It is the trusted friend in situations of doubt and difficulty in actual practice. The position it has attained in the scientific world, the impetus it has given to better modes of practice, are sufficiently revealed in the other works on our list, as, indeed, in all recent obstetric teaching. It is, however, but just to call attention to the issue of the third edition, and to notice the care with which many subjects have been worked out anew, and the fresh illustrations with which it has been enriched.

We will not pay the works of Leishman and Playfair the doubtful compliment of indiscriminate praise; but will accord them the honour they deserve of serious criticism. It is of the last importance in the actual relation of obstetric science to the other branches of medicine—a relation still of a somewhat militant character, that the books which represent obstetric science should be at least equal in all the essential points of literary and scientific workmanship to those which represent medicine and surgery. We should be guilty of grave neglect if we failed to do our best to point out the shortcomings of these books, the more especially as they are very susceptible of correction, because they are worthy of it, and because the authors possess, we believe, both the candour and the ability to correct and to bring their books up to the requisite standard.

To apply ourselves to our task. The plan we propose to follow is a little unusual. We shall point out certain literary faults, and in this connection refer to the subject-matter. For instance, commenting upon a diagram, we shall, taking this as a



text, offer occasional observations bearing upon the subject represented. In this manner, although we shall undoubtedly fail, as every review limited by space must fail, to do justice to the subject, to the authors of the works under examination, and to other authors, we may still hope to present a useful criticism.

It has occurred to us that the great modern institution of "Spelling-Bees" might be profitably applied to medical writers. Something of the kind is plainly wanted to supplement and to correct the glaring deficiency of our actual scheme of "Preliminary General Education." Grammar was at one time held to be the highest expression of Logic. It is certainly the feature of a book which is most open to general criticism, even by those who can form no opinion of the subject-matter. Hence men and women of fair education can apply a ready test by which to judge of the capacity of an author. If they find an author spelling and writing incorrectly, they will be apt to doubt his competency in other things. Foreigners, especially Frenchmen, whose genius is perhaps more strictly logical than ours, may not without reason conclude that authors manifestly deficient in the art of correct expression are so because they have no very correct ideas to express. A clumsy workman may spoil the best materials.

We are aware that we are treading upon very tender ground. There is nothing about which authors generally are so sensitive. But since many literary shortcomings are simply the consequence of carelessness, we feel bound, in the interest of English medical literature, to protest against a vice that not seldom supplies a ground for forming an inadequate estimate of our scientific merits.

In the first place, we feel compelled to notice instances where the names of authors are misspelt. For this there is no excuse. It is simply a question of copying; it requires no knowledge of foreign languages to keep right. We fear that the numerous faults of this kind that disfigure modern writings are accounted for partly, by the fact that the writers do not consult the authors whose names they misspell, but are content to quote at second-hand; and partly, because they neglect the obvious duty of revising the printers' proofs.

To cite a few examples:—Dr. Playfair quotes, p. 136, vol. i, "Hersh." Who is he? P. 137, "Hinchfeld." Does he mean Hirschfeld? At p. 139 we were somewhat perplexed by the name of "Larche." The matter in connection with the name suggests that "Larcher" is meant; at page 140, "Gassner" is designated under "Gaisner." At p. 149 we have "Beandelocque," and at p. 364 "Bandelocque." At p. 226, vol. ii, "Jorg" stands for "Joerg." "Litzmann" is only allowed

one "n;" this is generally the case with German names ending in "mann." At p. 155, "Keraradec" may be supposed to mean "Kergaradec." "Bacchetti" is spelt, p. 193, "Bachetti;" "Henry Bennet" would repudiate the second "t" which is given to him at pp. 213, 231. "Goubeyre" at p. 223 becomes "Gourbeyre" at p. 227. "Scacht," p. 238, is presumably "Schacht," or "Schatz." The latter name is quoted by Barnes in connection with "Retroversion," the subject under discussion; so that by inference we arrive at the real person. Vol. ii, p. 245, "Grundewaldt" probably means "v. Grünewaldt," since the topic associated with the name is the temperature in puerperal women. At p. 274 "Ferichs," the subject being albuminuria, stands probably for "Frerichs." Further on the name is given correctly. Consequently the uninformed student either is left in ignorance as to which is right, or he might conclude that two distinct persons are referred to. In vol. ii, p. 321, "Babnof" is quoted. We suspect this is wrong, but fail to recognise the person referred to, and there is no bibliographical reference to help us.

This carelessness about proper names entails serious evils. Not only is the student puzzled or misled; the authors unrecognised under their aliases are cheated of their just due, and the reader may be unable to correct or extend his knowledge of the subject referred to by consulting the original. Indeed, in some instances, as we have seen, it is only the skilled critic versed in the subject who is in a position to discover who is meant; and this is especially so when, as is too often the case in Dr. Playfair's book, no references are given to the works professed to be cited. When names are misspelt and references are suppressed, one cannot help suspecting that the authors' works have not been consulted.

Apart from proper names, numerous errors are scattered over the book. Thus, at p. 167, vol. ii, "pubis" is put for "pubes;" at p. 205, "spiculæ" is found; Leishman has the same at pp. 513, 516. At p. 55 we have "ricketty;" at pp. 55, 56, and in the index, we have "spondylolithesis," forgetful of etymology. A graver error, which common practice cannot justify, is the use of "forceps" as a plural noun. This is found throughout the book. It ought not to be necessary to remind men of ordinary education that "forceps" is a singular noun, gen. "forcipis." It does not become plural when imported into English because it ends in "s" and has two blades. This error is never seen in French or Italian authors. Leishman, Meadows, and Roberts correctly use the singular number.

Both Leishman and Playfair constantly speak of "oxytoxics" when they mean "oxytocics." "Oxytoxic" has no meaning

in connection with obstetrics. They cannot mean "swift-poisoning" or "swift-bowed." Using the familiar Hippocratic words "dystocia" and "eutocia," it is odd that they should fall into such a blunder. Either "dystocia" or "oxytoxic" is wrong. Playfair and Hippocrates cannot both be right. We cannot help but choose Hippocrates. Playfair, again, has "climateric" for "climacteric," p. 166, vol. i. At pp. 251, 252, the eye and ear are offended by "hydatiform," ignoring again the derivation from the genitive, which makes "hydatidiform." Vol. ii, pp. 304, 313, we have "midwives;" at pp. 312, 317, "diphtheria" and "diphtheritic."

Drs. Playfair, Roberts, and Meadows, with many respectable authors, write "Cæsarean." We should be pleased to know what is the authority, or rather the reason for this spelling. Authority, indeed, is easily found, but the reason has not been shown for what is clearly a departure from a well-recognised etymological rule. If the word be derived from "Cæsar," and Dr. Playfair refers to this origin, then the adjective takes the vowel of the genitive case. Cæsar, Cæsaris, gives Cæsarian. The Romans have Cæsarianus. The French, usually more correct in literary style than we are, have "Section Césarienne." The French preserve in their Latin derivations the classical forms. Their practice is therefore a better guide than that of the Italians, who sacrifice etymology to sound. Clerical errors are not nearly so frequent in Leishman. But "Deneux" is spelt "Deveux," p. 253; "Assolini" stands, p. 598, for "Assalini." Roberts has "Parré" for "Paré." It is needless to say that many of the errors specified are printer's errors, and Dr. Playfair is especially hardly used by his printer. The paper and type are good, but the compositor's work is decidedly bad. There are frequent instances of dropped letters and other blemishes which careful "reading" would have remedied. Still the author is to blame for trusting the printers, who always will let their 'devil' work out his wicked will upon proper names, and for not giving a list of "errata."

In the name of literary honesty we protest against the practice which Dr. Playfair seeks to excuse in the following passage from his preface:

"Many of the illustrations are copied from previous authors, while some are original. The following quotation from the preface to Tyler Smith's 'Manual of Obstetrics' will explain why the source of the copied woodcuts has not been in each instance acknowledged:—'When I began to publish I determined to give the authority for every woodcut copied from other works; I soon found, however, that obstetric authors of all countries, from the time of Mauriceau

downwards, had copied each other so freely without acknowledgment as to render it difficult or impossible to trace the originals.'"

Not even the great name of Tyler Smith can consecrate wrong. When we reflect that a whole theory, or the most important and original theory or operation, may be expressed in a diagram, we can understand how much injustice is done to the designer when his work is taken without acknowledgment. A graphic description is surely not less entitled to acknowledgment than is a verbal one. Scarcely one author in twenty is competent to form for himself a clear image, and to fix it in a drawing which shall reproduce that image to others. Those who do possess this faculty are beyond others gifted as teachers, and their work, whether expressed by pen or pencil, should be credited to them.

In point of fact, there are not many woodcuts in Tyler Smith or in Playfair which a little skill in the subject and a little industry would not easily trace to the rightful owners. And most of the rest are so bad that it would be better to omit them altogether. It is surely not a just plea to urge that because you do not know to whom to restore property you pick up in the street, you are therefore at liberty to pick a man's pocket; yet this is very like the argument borrowed from Tyler Smith. You are bound to take some trouble to restore what you find, and to acknowledge, if you cannot repay, what you borrow.

To give a few examples by way of justification of this criticism, which applies to all the text-books before us, and in the hope that it may lead to restitution.

Barnes is unfairly treated by Roberts. At p. 125, speaking of fatty degeneration of the placenta, Druitt, who worked and wrote under the inspiration of Barnes, is alone quoted. And Playfair, vol. i, p. 258, copies one of Barnes' drawings of fatty degeneration of the chorion villi without specifying the source. If the context be searched for guidance, this drawing would probably be attributed to somebody else, possibly to Dr. Playfair himself.

So, again, in vol. ii, figs. 118, 119, illustrating head-locking, borrowed from Barnes, would by inference from the context be likely to be ascribed to Reimann.

Since every new diagram seems liable to be copied and in time to make a part of current literature, it is desirable to condemn at once those which are bad. Fig. 65, Playfair, is designed to give "the size of the uterus at various periods of pregnancy." Now, this can hardly be done with any accuracy on the scale and in the manner of this diagram, and the direction of the lines representing the uterus gives a perfectly erroneous idea of

the position of the uterus. The axis is made perpendicular to the horizon! Fig. 88, again, showing "attitude of child in 1st position," makes the flexion of the head on chest excessive. Such a position would in all probability run into a shoulder presentation. Fig. 99 is a curious drawing "illustrating expression of the placenta." It looks as if the woman lying on her side inclining to the prone were expressing the placenta herself.

The vice of copying diagrams is almost sure to betray itself in the illustrations of breech-labours. Thus, Meadows, fig. 136, represents a position which is neither dorso-anterior nor abdomino-anterior, whilst the child's limbs are disposed in an altogether unusual fashion. Roberts' illustration, fig. 46, much better drawn, still fails to give the right position.

At the beginning of labour, that is, whilst the breech is still presenting above the brim of the pelvis, it is never disposed as these diagrams represent. At this stage the child's breech is either placed in an oblique or in the transverse diameter of the pelvis. The hips do not take the antero-posterior diameter until they approach the outlet. Playfair reproduces an old offender in fig. 200, open to the same objection as Roberts', and in common with Leishman's, fig. 117, it represents the legs extended in such a manner as would probably require the operation of bringing down one leg described and figured in Barnes, instead of being the normal type which it is supposed to represent. So long as these diagrams continue to be used it is not likely that students will ever acquire a correct appreciation of the phenomena of breech-labour.

Upon this subject of breech-presentations imperfect notions still prevail. Thus, Playfair, p. 361, vol. i, says—

"If the legs be extended on the abdomen, it will be necessary to introduce the hand and arm very deeply, even up to the fundus of the uterus, a procedure which is always difficult, and which may be very hazardous. Nor do I think the attempt to bring down the feet can be safe when the breech is low down and fixed in the pelvis. A certain amount of repression of the breech is certainly possible, but it is evident that this cannot be safely attempted when the breech is at all low down."

We would observe upon this that Playfair's argument is simply a *petitio principii*. It is contradicted by experience. The operation which Barnes describes as "decomposing the wedge" has been successfully performed a fair number of times. Upon its skilful performance the chance of bringing a live child mainly depends. For this chance must indeed be small if it be dragged through by traction on the breech by fillets or other instruments, even if in all cases this could be done. There are cases where "decomposing the wedge," difficult as it undoubtedly is, is really

easier than dragging out the un mutilated child breech-first. Under anæsthesia, with gentleness, patience, skill, and a correct idea in the mind of what has to be done, the operation is one of the most scientific and satisfactory in obstetrics. The last resource of Playfair, "embryotomy," can hardly ever be justifiable unless the case be complicated with pelvic deformity.

The dorsal or nuchal displacement of the arm is illustrated in the most singular manner by Playfair. Thus fig. 111 shows the child's head in front of the symphysis pubis! And fig. 112 is copied from a faulty drawing in Barnes' second edition, which this author has taken care to correct in his third edition.

The fatal habit of copying time-honoured plates is but too often the perpetuation of error. For example the illustrations of "spontaneous evolution or expulsion" in Roberts, Leishman, and Playfair taken from the same source, omit altogether the true first stage in which the head lies in one or other iliac fossa; they represent as the commencement that advanced stage of the process when the antero-posterior rotation of the child has been effected,—that is when the trunk has been driven into the pelvis. It is only then that the head comes over the symphysis pubis. In this way the relation of the mechanism of this form of labour to natural head-first labour is entirely overlooked. The grand unity of nature is ignored.

The diagrams in Roberts and Meadows illustrating "bi-manual" turning cannot be commended. They represent the inner hand of the operator which is supposed to act upon the presenting part of the fœtus in front of the symphysis pubis! Of course this is not intended. But it is nevertheless calculated to puzzle the student. The term "bi-polar" introduced by Barnes is much more scientific and descriptive than "bi-manual."

The diagrams of Hicks, moreover, give but an imperfect description of the process of bi-polar version. They stop short at the point where the knee is seized; whereas the bi-polar manœuvre still is called for to complete the version. This it does, as shown in Barnes' diagrams, by shifting the hands, the outer one now pushing up the head, whilst the inner one pulls down the breech. These diagrams are copied by Playfair, and should have been acknowledged.

Roberts, at p. 93, gives a diagram (fig. 55) "after Tyler Smith." Now this diagram Smith borrowed from Barnes. It embodies much of the latter author's theory of placenta prævia. *Sic vos non vobis*. He might as well have ascribed the theory the diagram expounds, as well as the diagram itself, to Smith.

Many of the woodcuts in Meadows' manual are simply hideous in their deformity. Nature in her most morbid moods never produced such monstrosities. Few of them possess

even the merit claimed for them of giving clear ideas—at any rate correct ideas. The diagrams 99, 100, 101, call for the remark that the vagina does not run in front of the symphysis pubis. It is true they are copied, but still by copying they are adopted. Other illustrations of turning (figs. 142, 143, 144), regarded as a series, are not instructive. In all we observe a fillet attached to the prolapsed arm. The object of this is not explained. We confess our inability to penetrate the mystery. The plates in Roberts are for the most part much better than those in Meadows.

On the subject of diagrams we may take the opportunity of remarking that all our authors, excepting Barnes, copy freely from Schultze's wall-diagrams. Now some of Schultze's drawings are good; but many are very faulty. The faulty ones are copied even more than the good. Thus Leishman takes his illustrations (figs. 95, 96) of retroflexion and retroversion of the gravid womb from the purely hypothetical and defective diagrams of Schultze. These fail entirely to show the most characteristic features of up-dragging of the urethra and meatus urinarius, and the bulging of the anus and perineum.

The drawings in Leishman are, however, generally executed with correctness and elegance. They show a more accurate anatomical perception and a better-trained artistic eye.

The theory of hæmorrhage and the modes of dealing with this terrible complication, are well discussed by both our authors. The distinct recognition of the physiological doctrine insisted upon by Barnes, upon which rests the rationale of the treatment by local application of styptics, namely, that there is a point where centric and reflex power to bring about contraction of the uterus fail, is a great advance in obstetric teaching.

The signs and diseases of pregnancy are upon the whole satisfactorily done. We can only stop to point out one or two errors. Leishman says calcareous degeneration of the placenta is commonly fatal to the child. This is far from correct. The minor degrees of calcification of the maternal surface of the placenta are not uncommon, but the child does not seem in the least affected; and even in the more marked cases, it is quite exceptional to find the child dead. The reason is that this change affects the decidua more especially, not involving the chorionic villi.

In giving the diseases of pregnancy, Leishman makes no attempt to group them under their physiological connections, but simply describes in an isolated manner the disorders of the several organs successively. It is an enumeration of diseases or phenomena, rather than a scientific exposition of

the pathology of the pregnant woman. The history of abortion is well treated by Playfair, less completely by Leishman.

The mechanism of labour is one of the best points in Leishman. He earned a well-deserved reputation by the accuracy of his observations and the keenness of his analysis of this subject. He has helped to make it easy by the precision of his descriptions. It is a masterly performance. We may, not without diffidence, urge that he has not sufficiently accounted for the final extension-movement of the head at the outlet. The chief factor is really the altered relation of the propelling force carried along the spine to the base of the skull, brought about by the head pursuing the pelvic curve. The consequence is that the spine is thrown out of its perpendicularity to the base of the skull, and so bears at an angle which is directed forwards. This, added to the increased friction retarding the descent of the occiput, compels the head to rotate on its transverse axis, the face coming down, that is extension. When the occiput is engaged under the pubic arch, the continuous propulsion acting more and more upon the anterior limb of the head-lever is alone enough to continue the extension.

The description of the occipito-posterior position is not altogether complete. In this position of the head Leishman fails to notice the factor insisted upon by Barnes, who demonstrates that the curve of the sacral promontory is the equivalent or analogue of the curve of Carus. Hence when the occiput descends the head tends to extension in following this curve, just as extension takes place under the pubic arch in Carus' curve in occipito-anterior positions. This is proved by the occasional transition into forehead and face presentations, and is admitted by the manœuvre advocated by U. West and Leishman for restoring flexion by pressing on the forehead. On the right understanding of this mechanism depends a just idea of the use of the forceps in these cases. The forceps acts by restoring flexion.

Upon this point we cannot help differing from Leishman when he says, p. 541, "we should always try to effect rotation by the forceps, previous to attempting direct extraction." Tyler Smith is more nearly right in urging "that we should rotate during the process of extraction." In some cases we have found this answer; but we must emphatically say from abundant experience that, in most cases, it is better to take no heed of rotation at all. Rotation is the result of the natural adaptation of the head to the particular part of the pelvic canal in which it comes. So, as the head descends under the *vis à fronte* of the forceps, it will adapt itself. This law is at once more



true and more simple. Artificial rotation is an example of the *nimia diligentia medici*.

All our authors seem to be hesitating before committing themselves frankly to the more advanced practice in the use of the forceps. But they will certainly free themselves from the trammels of tradition. We feel confident that their practice is already better than their teaching. Playfair subscribes to the old law, "The position of the head should be accurately ascertained" before applying the forceps; yet, at p. 182, he admits, quoting Barnes, "that it is unimportant." His diagrams, figs. 151, 152, are not accurate or very intelligible. Fig. 151 is perplexing through the introduction of four hands; and in fig. 152 the blades showing "traction" would assuredly slip; the head is not grasped beyond its equator. Nor is Leishman exempt from criticism on this point. Fig. 151 "shows introduction of upper blade," the point at the vulva pointing forwards, an almost impossible way of passing it; and he introduces three hands, one holding the lower blade, a needless complexity. The surgeon can very well do this with the back of one hand, whilst using both to insert the second blade. Indeed in most cases he must do his work without help, and if he cannot the sooner he learns the better. Leishman, too, like all who use straight forceps, advises that the blades should be applied to the sides of the head; and of course says that "the buttocks must be brought to the edge of the bed." This last injunction is not always easy to obey; with the double-curved forceps it is not necessary; and in the endeavour to obey the first the operator will commonly be frustrated unless the head be near the outlet. By and by it will be recognised that it is the pelvis that governs the application of the forceps more than the head.

Leishman figures, p. 593, a craniotomy-forceps, which we caution the young practitioner to reject. It is the old villanous instrument with fixed joint and spikes inside the blades. Such an instrument is difficult to apply in cases of marked contraction, and when applied it will not hold well; it is apt to tear away. The spikes are a bungling substitute for good workmanship. The blades should hold by accurate parallelism, as in the models of Simpson, Barnes, Hall Davis and Matthews; and the blades being separate are more easily adjusted.

Playfair rightly extols the cephalotribe; but his description and diagram suggest that he can hardly have mastered the principle of its action. At p. 205 he says, "in order that the base of the skull may be reached and effectually *crushed*," &c. Now, as a matter of fact, it is very rare indeed for the base to be crushed; as soon as its edges are caught between the blades, the base almost infallibly cants, turns over, so that the remains

of the cranial vault are flattened down upon the base, the whole forming a kind of disc, and thus it passes through the pelvis edge on, like a plate. The diagram (fig. 165) gives no correct idea of the action of the cephalotribe. Leishman entertains the same erroneous view, that the object of the cephalotribe "is to *crush* the unyielding base into a pulp."

Success in appreciation of the operative procedures in labour with contracted pelvis must be based upon a right understanding of the mechanism of labour under this condition. A fair account of this is given in Schroeder. This account Playfair quotes, ascribing the merit of it to the German school. But it is really much better done in Barnes. The key of the whole process is what this author has described and figured as the "curve of the false promontory;" the projecting promontory representing a point around which the head must revolve before it can enter the pelvic cavity, and enter the orbit of exit or "Carus' curve." All extracting force must follow, first Barnes's promontorial curve, then the pubic curve. Due observance of this law wonderfully facilitates extraction, and saves the mother from injury. In head-last labours it is of paramount importance. It is from neglect of it that the trunk is pulled away from the head.

Playfair speaks, it seems to us, with imperfect appreciation of the operation of "bisection," as it was called by D. D. Davis, a form of which is decapitation. Speaking of cases where turning is difficult or impossible, he says (p. 388, vol. i): "Should all these means fail, we have no resource but the mutilation of the child by embryulcia or decapitation, probably the most difficult and dangerous of all obstetric operations." We can answer for it that in some cases, at any rate, decapitation is one of the most easy and safe. It is entirely scientific and satisfactory. Rightly done, all the violence falls upon the dead child. The mother is absolutely respected. In some cases undoubtedly it is difficult to get a hook or wire or whipcord round the neck. In such cases division of the spinal column by strong scissors is our resource.

Playfair, in like manner, entertains an undue estimate of the difficulty of extracting the decapitated head left in utero. "Perforation," he says (vol. ii, p. 210), "is by no means easy, on account of mobility." He does not describe the operation. If the head is properly fixed by the two hands of an assistant upon the pelvic brim there is no difficulty in perforation; and then the head is readily extracted, either by the craniotomy forceps or by cephalotribe. It is an affair of ten minutes. In discussing "decapitation," Leishman omits all notice of the very important alternative sometimes called for, of bisection of the

trunk, or spondylotomy. Nor is Leishman very happy on this point. He thinks the operation difficult, and prefers the midwifery-forceps after perforation. We hope he will try the craniotomy-forceps, not the one he has figured, but a good one, and he will find the operation easy enough.

Discussing the conditions for Cæsarian section, Leishman contends, quoting Dubois, for a minimum conjugate of two inches. But he ignores the obvious fallacy that lies in comparing the dicta of men who worked thirty years ago with bad instruments with the experience of those who now work with improved instruments. Dubois himself would have admitted the expediency of delivery after craniotomy below two inches if he could have accomplished it. This is one of the things practitioners have to learn how to do, and not to rest content with a dogma laid down under conditions that have passed away.

Induction of labour is fairly discussed by both our authors; but we cannot understand the necessity Playfair points to, of passing the hand into the vagina for the purpose of introducing the bougie into the uterus. We have never found occasion for this. We always like to see ideas supposed to be settled attacked now and then. It compels revision of the grounds upon which they rest, and even if the attack fail to disturb the old conviction, it cannot fail to make the conviction a more rational one. It has been settled for a hundred years that the induction of labour is a proper thing to do when there is such contraction of the pelvis that a living child at term cannot be born *per vias naturales*. But latterly, Spiegelberg and Litzmann contend that it is better and safer to the mother to leave the labour to come on at term, and that the risk to the child is so great in artificially induced labour as to lead to the conclusion that the operation should be altogether abandoned, except perhaps in the extreme distortion in which the Cæsarian section might otherwise be necessary. Of course this heterodox view is supported by statistics. Upon this we would observe that English experience would hardly show any increase of risk to the mother; whilst if even a few children are saved, the balance of life is entirely in favour of the operation. If in Germany the operation is so fatal to the mother, we are compelled to conclude that there is great room for improvement in practice in that country. We have had but one death, and that was in a case of extreme osteomalacic deformity, brought into a hospital, where diphtheria attacked the patient. That the German objection should be endorsed to a modified degree by Duncan is, perhaps, not surprising. He has at least that part of Simpson's merit of scepticism carried into practice which

led his predecessor to rip up every settled conviction. We concur with Playfair in admitting that the operation has been performed more often than was absolutely necessary; but this does not invalidate the general argument. And when Duncan enforces his objection by insisting on the rarity of great distortions, emphasising it by declaring that a case of extreme distortion has hardly been known in Scotland within the memory of man, he does not really meet the question. That distortions do occur out of Scotland, at any rate, is too certain; and it is not less certain that hundreds of men quite competent in the matter testify to the fact that, after being compelled to sacrifice one or more children, they have happily brought living children into the world, the mothers too having encountered less risk than when delivered at term.

The case is well stated by Playfair, and decided with excellent judgment.

In describing the diseases of childbed, Leishman shows the same want of grasp that we notice in dealing with the diseases of pregnancy, in bringing together disorders that spring more or less directly from common conditions, and which, though differing in some of their manifestations and issues, are yet naturally associated. Playfair has seen this relation a little more clearly. Both authors contributed excellent material to the discussion on puerperal fever at the Obstetrical Society; both have studied the subject with great care, and both have set forth the case with clearness and ability. Dr. Leishman's statement is remarkable for the candour with which he revises the doctrines he laid down in his first edition. High as was our respect for his character before, we feel that he has earned a just claim to increased confidence as a teacher and practitioner by his readiness to re-examine and correct his published opinions.

Dr. Playfair's description of puerperal fever is a remarkably able statement of the most recent doctrines. He adopts the division into autogenetic and heterogenetic cases, but omits to assign this classification and the terms which express it to Dr. Barnes, the originator. A very excellent chapter in Playfair is that in which he describes the state of women after labour, and the management of puerperal women. The same praise is due to the description of the new-born infant and the care it requires. Nothing could be more useful to the student and young practitioner.

In conclusion we may congratulate those who teach, those who practise, and those who are learning the science and practice of obstetric medicine, that we possess two manuals that fairly represent the actual state of our knowledge, that may

be generally trusted for the information they contain, and for the judgment with which it is set forth.

Notwithstanding the literary blemishes we have found it incumbent upon us to point out, it is no more than just to say that both works are written with considerable ability. The style of Leishman is graceful, clear, orderly, and flowing. It is most pleasant reading. Dr. Playfair's capacity for seizing a subject and placing it in a clear light before the reader is conspicuous. This and the other merits which characterise his book will remain; the blemishes will disappear.

### VIII.—Food: its Adulterations and Analysis.<sup>1</sup>

THAT the health and physical well-being of a community are matters bound up with the food of the people is an incontrovertible proposition, and its truth is nowhere more apparent than in the English-speaking race. The differences between the squalid, typical wild Irishman, and the Irishman well fed and prosperous in England and America, are matters of notoriety, and in no small degree due to differences of food. The Dorsetshire labourer with his scanty fare is incapable of executing the same amount of work as the better fed, and consequently more stalwart, Norfolk or Yorkshire labourer. That this difference is mainly one caused by food is shown in this, that the worse-fed south country farm-labourer when he migrates, and becomes the beef-eating navigator in a more northerly region, is by no means manifestly inferior in physical capacity and endurance

<sup>1</sup> 1. *Food: its Adulterations, and the Methods for their Detection.* By ARTHUR HILL HASSALL, M.D. Lond. London, 1876, pp. 896.

2. *Nouveau Dictionnaire des Falsifications et des Altérations des Aliments, des Médicaments, &c.* Par J. LEON SOUBEIRAN. Paris, 1874, pp. 634.

3. *Dictionnaire des Altérations et Falsifications des Substances Alimentaires Médicamenteuses et Commerciales.* Par M. A. CHEVALLIER, en collaboration avec M. E. BAUDRIMANT. Paris, 1875, pp. 1260.

4. *Adulterations of Food, with Short Processes for their Detection.* By ROWLAND J. ATCHERLEY, Ph. D., F.C.S. London, 1874, pp. 112.

5. *Die Chemische Werthbestimmung einiger Starkwirkender Drogen.* Von Dr. G. DRAGENDORFF. St. Petersburg, 1874, pp. 126.

(*The Chemical Valuation of some of the more Potent Drugs.* By Dr. G. DRAGENDORFF).

6. *Milk Analysis.* By J. ALFRED WANKLYN, M.R.C.S. London, 1874, pp. 70.

7. *Tea, Coffee, and Cocoa Analysis.* By J. A. WANKLYN. London, 1874, pp. 59.

8. *The Commercial Handbook of Chemical Analysis.* By A. NORMANBY, enlarged by HENRY M. NOAD, Ph. D., F.R.S. London, 1875, pp. 480.

to his northern brother. And this, too, spite of the adverse influences of a bleaker and less favourable climate. The stalwart Highlander, with his abundant diet of nutritious oatmeal, is also an instance in point.

The advantages of an ample supply of food having been recognised, and the shameful frauds perpetrated a quarter of a century ago by unprincipled vendors having attracted the attention of government, a necessity began to be strongly felt for some legislation to put a check to the adulteration of articles of food and drink; and this led, in 1860, to the passing of the first Adulteration Act. Not that no previous attempts had been made to stem the tide of adulteration, but these previous measures had as their chief aim, not the protection of the public health, but the prevention of frauds on the revenue. Thus a statute of Henry III forbade the sale of unwholesome wine and meats, and two statutes of Charles II and William and Mary respectively, imposed heavy penalties upon persons adulterating wine. Various Acts of Parliament, dating from the year 1820 downwards, dealt with the adulteration of bread; whilst other statutes dealt with adulterations of tea, coffee, cocoa, and beer. The year 1860 however may be regarded as that in which for the first time any serious attempt was made to restrain adulteration by a general penal enactment. The evidence which had paved the way for the Statute of 1860 was ample and conclusive. The existence of wide-spread adulteration, too often of a deleterious character, had been fully proved, and though a good deal of unwholesome sensation had been mixed up with the agitation, no one can doubt that the Act of 1860 was called for, and that stern repressive measures were needed. The Parliamentary Committee of 1855 had reported that it "cannot avoid the conclusion that adulteration widely prevails." "Not only is the public health thus exposed to danger, and pecuniary fraud committed on the whole community, but the public morality is tainted, and the high commercial character of the country seriously lowered both at home and in the eyes of foreign countries." The Lancet Analytical Sanitary Commission, crude and open to question as were some of its conclusions, had done excellent service, and the result was a statute that was supposed to be beneficial.

Unfortunately, the Act of 1860 was, like much other sanitary legislation, permissive; the Act might or might not be put in force in a district, consequently it remained practically a dead letter, and no effective proceedings were even instituted against fraudulent tradesmen. It was then the more remarkable, in 1868, when the necessity of putting an end to the still more shameful practice of adulterating drugs came under the

notice of the legislature, that a clause was introduced into the Pharmacy Act of that session, attaching a penalty to any adulteration of drugs, whether the added substance were or were not of a noxious character.

But though the Act of 1860, and the adulteration clause of the Statute of 1868 were virtually dead letters, it must not be concluded that they were altogether ineffective. Whether it was that their provisions were so highly penal that adulterators did not dare to brave the peril of a prosecution, or that the dangerous consequences attaching to adulteration with poisonous substances came home to the adulterator, certain it is that adulteration with injurious ingredients gradually almost died out. This is proved beyond controversy by the results of the analyses made under the Act of 1872, for among the hundreds of prosecutions instituted under this Statute, very few indeed were for injurious admixture if the adulteration of bread with alum be excepted.

Nevertheless, the admixture of materials in themselves harmless, yet of inferior commercial value, with more costly substances still prevailed; and among commercial men this practice was not thought to detract from the character of the parties acting thus. Nay, the practice was even defended, it being found convenient to invent some fiction by which the fraud on the pocket was put in the guise of an improvement of the article. Coffee of low quality and flavour was "improved" by the addition of chicory, if of nothing worse; but the vendor forgot to state that the coffee "improved" was of inferior value, though a high price was charged. Arrowroot was debased by admixture with, or entire substitution of, inferior starches. Cocoa was rendered "soluble (save the mark!)" by being mixed with coarse sugar, inferior sago, rice sweepings, and other amylaceous rubbish, till the "cocoa" was little more than cocoa in name. We ourselves had once the misfortune to need a prompt and efficacious emetic. Mustard was accordingly resorted to; but, alas! the "London" mustard of a well-known firm was used; tablespoonful after tablespoonful was administered, but without effect; and eventually other means had to be taken to produce the desired result. We did not then know what we subsequently ascertained, that "London" mustard is usually of very inferior quality, being more largely adulterated with wheaten flour and turmeric than any other matters, and that what it loses in pungency and flavour by the dilution is compensated for by the addition of the pungent red pepper.

In 1872 a new Adulteration Bill was pushed through Parliament, almost surreptitiously, at the far end of the session. Had

there been any serious expectation that the Bill would pass, more effective means would, without doubt, have been taken to nip it in the bud. And, indeed, those who were most ardent in the cause of anti-adulteration were by no means sanguine as to the successful working of this crude and imperfect piece of legislation. Very much to the astonishment of most people, the measure was found to be very effective—too effective, indeed, for the interests of trade, which required time to adapt itself to a new and more rigid code of morality enforced by penal provisions. The sale of an adulterated article as unadulterated was soon found to be an expression having a wide significance, and traders speedily found that the shoe pinched. We have a vivid recollection of being told by a gentleman notorious, if not eminent, in trade, one day, soon after the passing of the Act of 1872, that the requirements of the analysts to be appointed under the Act should be few; all that was necessary in the analyst was that he should have a general and not very refined knowledge of analytical chemistry, and be acquainted with trade. This very gentleman, a few months later, when his own peculiar branch of trade was touched to the quick, waxed eloquent on the incompetence of analysts. The necessity was keenly felt of having more highly qualified men, by which was meant Government officials with a smattering of chemistry, and with a leaning towards the weaknesses of "the trade." We ourselves have always thought that the alleged incompetence of the analysts appointed under the Adulteration Act of 1872 has been over-stated. Gross incompetence has been rarely exhibited; and though, no doubt, great expertness in the more delicate branches of analytical chemistry is rare, good sound work has, we believe, been the rule and not the exception, as some would have us believe. The fees paid for analyses were, as a rule, too low, and these were but rarely supplemented by any adequate salary or retaining fee; nevertheless, some chemists of acknowledged position, skill, and merit, consented to fill some of the appointments under the Act.

The hostility excited among the members of the trading community, especially the grocers, rather than the incompetence of the public analysts, led, in a short time, however, to the appointment of a Select Parliamentary Committee, which sat during the summer of 1874, to inquire into the operation of the Adulteration of Food Act, 1872. This committee reported that the Act itself was defective and needed amendment; that whilst the number of proceedings outside the metropolis and a few large towns had been singularly small, the deterrent effects were undoubtedly great, and the opinion of the promoters had been substantiated, that the most beneficial effects of the Act



would be to prevent adulteration rather than to punish it. As showing the good effects of the Act, the committee reported that it would afford some consolation to the public to know that in the matter of adulteration they were *cheated* rather than *poisoned*. We question very much whether the public did receive solace from this assertion, for we believe that the average low-intelligence Englishman objects, above all things, to being cheated, whilst he does not seem to pay much heed as to whether he is being poisoned or not.

Whilst *ex parte* witnesses declared before the Select Committee of 1874 that the failures and hardships in carrying out the Act had been chiefly due to the incompetence and inexperience of the public analysts, the Committee—on which the traders were excellently well and fully represented—refrained from endorsing this condemnation. They thought, indeed, that in some cases a decided want of chemical knowledge had been proved, but no more than was to be expected from the sudden call made for the services of adepts in a branch of chemistry which had not been previously very highly valued. Nevertheless the Committee thought that the injury inflicted, and the undeserved penalties imposed upon some respectable tradesmen in carrying out the Act were due not so much to this cause as to the want of a clear understanding as to what did, and what did not constitute adulteration. The result of the deliberations of the Select Committee was that the Adulteration Acts of 1860 and 1872 were repealed, a clean sweep of the term adulteration was made, and a new Statute—the sale of Food and Drugs Act, 1875—was enacted. The working of this Act it is not now our intention to review. We shall rather confine ourselves to the historical and scientific parts of the question and make some reference to the existing state of knowledge relative to the existence and means for the detection of adulterations.

After the startling disclosures of the 'Lancet' Analytical Sanitary Commission were published in 1851-4, the reports of the Commission were collected into a volume entitled 'Food and its Adulterations,' and its well-known author, Dr. Hassall, subsequently supplemented it with another work, 'Adulterations Detected,' a book which professed to furnish plain instructions, microscopical and chemical, for the discovery of adulterations in food and medicine. To Dr. Hassall, by whom the major part of the analyses of the Commission were made, appertains the great merit of having proved the value and shown the use of the microscope in the detection of a large number of adulterations, the existence of which could not well be ascertained by purely chemical means. Thus, for example, the adulteration of arrowroot with potato starch cannot be

certainly demonstrated without the aid of the microscope. Again, although chemistry is most serviceable in enabling the chemist to estimate the extent or amount of admixture of chicory with coffee, the debasement with chicory is a fact which cannot well be ascertained without the use of that instrument. It was also due to the readiness and facility with which many kinds of adulterations are detected by the microscope that Dr. Hassall was able to make a very large number of analyses within the space of four years, for although he termed his analyses "microscopical and chemical," the chemical analyses were completely overshadowed by the microscopical.

At the time of the passing of the Adulteration Act of 1872, no work existed in the English language that could possibly serve as an adequate text book to guide the food analyst in his practice. The two volumes of Dr. Hassall to which we have referred were out of print; and though the microscopical descriptions and the somewhat crude woodcuts of 'Adulterations Detected' were excellent in their way, and the best of their kind published, the chemical portion of this work was very untrustworthy, and, indeed, had never been highly thought of by chemists. Nevertheless, 'Food and its Adulterations' had no strong competitor, and it served as a crude basis for instruction in food analysis. Very naturally, then, a new edition was here anxiously looked for, and we have before us 'Food : its Adulterations, and the Methods for their Detection' from the pen of the same author. Dr. Hassall states in his preface that, although in the present work he "has followed somewhat the method adopted in his previous books on the same subject, yet the volume now published contains a large amount of additional matter, and that nearly the whole of the articles which are not entirely new have been much extended or entirely rewritten."

We wish that we could find in the book itself sufficient evidence of the entire accuracy of the above statement. The book is, no doubt, made bulky by the addition of new articles; but these, with the exception of one on 'Water and its Impurities,' are unimportant, and the whole of the articles on drugs contained in 'Adulterations Detected,' of which 'Food' is but a second edition, are omitted in this latter volume. Much valuable matter is contained in Dr. Hassall's book; but the work is diffuse to a degree, ill-arranged, and filled with much obsolete matter. Page after page is taken up in tiresome iteration with inquiries as to the capabilities of the Excise to detect adulterations founded on evidence taken before the Parliamentary Committee of 1855. Surely the reader's patience might be spared these long attacks upon the professional cha-

racter of a gentleman whose connection with the Somerset House Laboratory ceased long before the appearance of Dr. Hassall's book, and against whose successor and subordinates no proof of incompetency is adduced. An attack founded on evidence now more than twenty years ago, at a time when the detection of adulterations was still in its infancy, is not creditable.

The methods given by Dr. Hassall for the determinations of adulterants are too often slovenly, and not to the point. We will cite two instances only in support of this assertion, though these instances might be almost indefinitely multiplied. No means whatever are described by which the quantity of chicory mixed with coffee may be even approximately determined, though this must be an everyday operation with food analysts. Again, although after an enumeration of the substances used in the facing of tea, the author states that the detection of all those substances is 'by no means difficult, we venture to assert that he specifies no means by which anyone could detect them with certainty and readiness. Nevertheless, many of the processes given are good, as, for instance, those for the analysis of wine. It would have been well, nevertheless, to have stated that these are taken almost bodily from the well-known work of Thudichum and Dupré, and are known to be, many of them, due to this latter able chemist.

The whole book lacks point and emphasis, and it might well and advantageously be compressed into half its present bulk. We should also suggest an excision of the highly objectionable series of advertisements, extending over more than fifty pages at the end of the work. "Pure" wines and "farina vitæ" here appear on miniature wall posters; and, what is worse, many of these advertisements are backed up with puffing certificates purporting to be signed by Dr. Hassall himself. Such certificates are less than valueless; they may serve as handles for the perpetration of shameless frauds. Such a certificate once given, there is no guarantee that the most worthless article may not be passed off under its protection.

There is a peculiar interest attaching at the present time to the composition and analysis of malt beverages. The hop-growers of Kent have recently taken alarm at the extent to which, owing to their own action, other bitters are now substituted for hops. The proper composition of malt liquors may hence claim some notice for us here. The honour of having invented ale is, of course, ascribed to Bacchus, in Egypt, where the climate, and perhaps the soil also, forbid the cultivation of the grape. The origin of the art of brewing is ascribed to Isis, the wife of Osiris; and Herodotus mentions Ζῦθος, a beer

obtained from barley. It is indubitable, at any rate, that beer was drunk centuries before the Christian era. The modern term "barley-wine," for a kind of ale, is adopted from Theophrastus, and that the beverage was alcoholic is shown by Aristotle's use of the term "beer-drunkenness." Not only the Greeks but the Romans learnt the art of brewing from the Egyptians, though it is doubtful whether the "trade" ever gained such power in ancient Rome as in modern London. The ancient Gauls and Germans also brewed, and, either from them or the Romans, the art of brewing passed to our own island. Alehouses are mentioned so early as A.D. 680, and alebooths were regulated by law in A.D. 728. It is not, however, till the 15th century that we get much information as to the composition of ale, and almost our first information is respecting its sophistication. Ale was then made of malted barley and yeast alone, whilst beer was probably flavoured with wormwood and other bitters; but at a still more remote epoch, beer and ale were synonymous terms, and, moreover, the beverage was not flavoured with bitters. The term beer appears to have been gradually limited to the bittered article. In A.D. 1482, beer was so highly valued that it was made a capital offence in Scotland to mix wine with it. It was not until A.D. 1524 that hops were introduced, and great indignation appears to have been aroused at the use of this "noxious weed" in brewing, a fact which we commend to the notice of our hop-growing friends.

In A.D. 1660 all malt liquors were placed under the control of the Excise. Up to the year 1847 nothing but malt and hops were permitted to enter into the composition of beer, but an Act was then passed enabling brewers to use sugar under certain restrictions. Heavy penalties attached to brewers, dealers, or retailers of beer, having in their possession any substitute for hops or malt; and even to druggists supplying such articles to a brewer. With the repeal of the duty on hops came however, in 1862, a repeal of the above enactment so far as it related to hops, and as the law has since stood, a brewer may use quassia or any other simple bitter as a substitute for hops; but the prohibition still continues with respect to substitutes for malt, so that it is illegal to use any substance giving pungency or conferring intoxicating qualities—such as *cocculus indicus*, grains of paradise, and tobacco. An abortive attempt was made in the Licensing Act of 1872 to prevent the introduction of certain deleterious substances into malt liquors, but this schedule of noxious ingredients was subsequently withdrawn. Seeing, then, that malt liquors may legally be brewed not only from malt but also from glucose, and that they may be flavoured with hops, or any other bitter which does not at the

same time confer pungency and intoxicating properties, it is surprising to find Dr. Hassall defining adulteration of malt beverages as—

“Any other substances than the constituents of malt and their derivatives, hops and water in such proportion as in the case of stout, strong and pale ale, to reduce the absolute alcohol to less than 4·5 per cent., and in porter and beer to under 3·5 per cent. Although the law allows the addition of both sugar and salt, we regard these additions as adulterations” (p. 669).

The brewers were not slow to avail themselves of the removal of the restrictions on the use of bitters, and at the present time hops are largely replaced by so-called “hop-substitutes,” of which there are many kinds known in trade, mostly having quassia as their basis. The quantities of these substitutes now used is enormous, and we have it on the authority of an eminent hop merchant that in some breweries hops are now almost entirely superseded by “substitutes.” But even prior to the removal of those restrictions, there is ample evidence to show that, in order to adulterate beer, brewers and retailers were ready to run all risks, and, to its shame be it said, the Excise was quite incompetent to put a check to the practice of such adulteration. Mr. Wickham stated before the Parliamentary Committee in 1855 that the adulteration of beer was then very common, so much so that the exception was not to adulterate. Dr. Hassall in his ‘*Adulterations Detected*’ (1861), enumerates among the adulterations of beer—water, sugar, treacle, salt, green vitriol, quassia, gentian, camomile, ginger, coriander and caraway seeds, capsicum, grains of paradise, liquorice, alum, sulphuric acid, salts of tartar, carbonate of soda, cocculus indicus, tobacco, and, on dubious grounds, opium and strychnine. But this last substance has never been proved to be used in the adulteration of beer, nor is it likely to be so. The only real evidence on which the statement rests is contained in a book entitled ‘*Brewing Malt Liquors*,’ published many years ago, wherein, among other receipts for brewing, the author, Mr. Morris, gives one into which “*fabia amara* (nux vomica)” enters as a constituent. We are sorry then to observe that in his most recent work Dr. Hassall enumerates strychnine as a substance which had been known to be employed in the adulteration of beer (p. 689), though he subsequently states (p. 693) “that the statement made concerning the use of strychnine in beer is scarcely consistent with probability.” This is but one among very many similar assertions respecting the adulteration of articles of food and drink with specified substances on altogether insufficient foundation. We venture to think that the list of adulterants of malt liquors which the

learned doctor puts forth in the work we have placed at the head of this article might, with truth, be materially curtailed. It differs in no essential respect from the list we have already quoted from his earlier work, except in the addition of picric acid, a poisonous substance which is frequently spoken of, but now never found in British beer. Indeed, when we come to inquire into the adulterants *detected* by Dr. Hassall in beer, we find salt, cane sugar, treacle and water only! Not that we by any means believe that the other adulterants specifically mentioned by him are never used, for some of them we well know are largely employed by brewers. The deficiencies of our author are, however, lamentably apparent here. No mention is made of the hop substitutes now manufactured and sold by tons weekly; and no means are described for the detection of some of the most common materials, *e.g.* quassia, used in brewing, whilst a whole page is occupied with a well known process for the detection of strychnine and opium, neither of which is ever known, in this country at least, to be used for the adulteration of beer, except opium for the purpose of drugging.

The adulteration of drugs is a subject entirely omitted from Dr. Hassall's book. The subject is one very cursorily treated in any manual of analysis, and the student who wishes for information must resort to manuals of materia medica and to pharmacopœias, in which the instructions for the detection of sophistications are usually meagre and unsatisfactory. We hail, therefore, the appearance of Dr. Dragendorff's excellent little brochure with great satisfaction, as an attempt to determine the value of the most important drugs by correct chemical methods. He treats of belladonna, conium, hyoscyamus, aconite, ipecacuanha, nicotine, colchicum, nux vomica, guarana, tea, coffee, opium, chelidonium, aloes, and cantharides, and the preparations made from these substances. He also gives full instructions for the chemical valuation of these, chiefly by the estimation of their contained alkaloids. Dr. Dragendorff, one of the greatest of our living toxicologists, who holds the chair of pharmacy in the Russian university of Dorpat, deserves the thanks of the pharmaceutical and medical professions for his excellent pamphlet. We trust that the example he has set will be followed by other pharmacutists, and that we shall be favoured with many other works of a similar nature.

The chief articles to which public analysts have as yet directed their attention have been bread, milk, butter, and tea; whilst wines, beer, and spirits appear to have received less attention, probably in consequence of a mistaken notion that alcoholic liquors were exempted from the operations of the Act of 1872. We hope that this omission may soon be rectified,

for though we believe that at the present time few positively hurtful substances are used for the adulteration of intoxicating beverages, a large amount of "doctoring" goes on. A well-to-do East End publican, himself at one time an analytical chemist, recently told us that all the profit he and his fellow retailers obtained by the sale of spirits was by diluting them with water. It has also been, no long time ago, successfully contended before a bench of country justices that a publican may add water to gin till the water forms 99 per cent. of the mixture without coming under the penal clauses of the sale of Food and Drugs Act. Perhaps in the interests of the public health it is well that this should be so; but in the interests of public morality it is not well, and we do not hesitate to say that it is a disgrace to the licensed victuallers as a body that such practices should be rife. Need we add how, in the face of these facts, the task of prescribing alcohol in definite quantities becomes one of extreme difficulty to the physician.

The relative strengths in alcohol of the malt liquors of former times and of the present day is a matter of considerable importance to which attention has recently been directed. Unfortunately analytical data of sufficient extent are wanting to enable us to determine this point with precision. We should imagine that the laboratory of Somerset House must possess a large amount of information bearing upon this question; and the Excise authorities may, perhaps, under parliamentary stimulus, be induced to publish it. But at present, we repeat, no sufficient number of analyses have been published to permit us to draw any inference as to the increased or decreased alcoholic strength of the people's beverage now as compared with, say, a quarter of a century ago, much less with more remote epochs. Some highly interesting statistics have, however, been published in the 'Times' within the last few months bearing upon the point we are discussing; and these, when coupled with changes in the law relating to the manufacture of malt liquors, will be seen to tend much to elucidate the subject.

It stands recorded that in the year 1860, 41,754,050 bushels of malt were retained in the United Kingdom for home consumption, charged with the duties of excise. This is equal to 1.4 bushel per head of the estimated population of that year. In 1874, the consumption had risen to 62,817,295 bushels of malt; or 1.9 per head of the estimated population. In 14 years then, there was an increased consumption of half a bushel per head, which is an increase of 36 per cent. Nor was this greater consumption of malt by any means accounted for by an increased use of malt for the manufacture of ardent spirits; for the quantity of spirit subject to excise and retained for home

consumption did not increase so much as that of malt in the same period. The increase was in fact from 21,404,088 gallons in 1860 to 30,321,928 gallons in 1874, an increase from 0·7 gallon to 0·9 gallon per head of the estimated population, or 28·5 per cent. per head. Thus the increase in the home consumption of malt has exceeded the increase in the home consumption of spirits. It is probable, too, that during the period referred to there has been an increased consumption of unmalted grain in the manufacture of spirits, leaving more malt to be used in brewing. It is hence perhaps premature to take up the cry of some alarmist that the beer of the present day is made from less malt than formerly. It is true that with the relaxation of the law, by which it is now permissible, under certain restrictions, to use sugar in brewing, the amount of this substance rose from 92,415 cwt. in 1860, to 828,430 cwt. in 1874, an increase of nearly 900 per cent. The legitimate inference from these statistics is, not that less malt is used in the brewing of beer, but that more malt, and much more sugar, are used in the manufacture of the beer of the present day; and hence that our national beer is more potent than formerly.

The malt liquors sold in London are we know from our own observations more alcoholic than is commonly supposed, or than the older published analyses would lead us to suspect; and it is probable that the intoxication so often attributed to adulterants is more commonly due to excessive imbibition of a potent alcoholic beverage. We believe that the British public nowadays consumes not only more, but stronger drink than formerly. It is a melancholy reflection that the increased consumption of food, and food of a superior quality, which free trade has enabled the people at large to indulge in has not been followed by a decreased consumption of alcohol, but by a large increase. Whilst larger and better feeding has been a great good, we doubt whether the increased consumption of stronger liquors has been an unmixed benefit. It can hardly be doubted that the old Adulteration Act of 1872, and the existing sale of Food and Drugs Act of 1875, have resulted in an improvement in the quality of the chief articles of food and drink, and a comparative freedom from noxious admixture. As chemists improve their analytical processes and the means of detecting and quantitatively estimating adulterants, still greater improvement in the quality and purity of the chief articles of domestic consumption may be expected. In recently looking through the quarterly reports of a well-known public analyst for a large district, we were struck with this: that the percentage of adulterated articles had quarter by quarter steadily decreased, that the percentages of adulterants added had



likewise diminished, and that the use of deleterious adulterants has almost disappeared. We learnt also, on inquiry, that the quality of the articles bought for analysis had greatly improved of late, and that these improvements were chiefly noticeable in the chief articles of domestic consumption—milk, bread, tea, and coffee.

Formerly milk, in towns at least, was almost universally mixed with water, and to such an extent that the article was milk in name only. Forty, fifty, sixty, and even seventy per cent. of the stuff as sold was added water; and not only were the grossest frauds thus perpetrated, but the health and lives of children and invalids were seriously jeopardized by the practice of this villany, which was rendered still more atrocious by the fact that most of the cream was also abstracted from the milk before sale. Nowadays, owing to the vigorous action of local authorities, their inspectors, and analysts, the milk of our large towns, though not absolutely pure, contains as a rule, not more than ten or twelve per cent. of added water, and not more than one third of the cream has been skimmed off. Indeed, the skimmed milk of the present day in the metropolis is, we are told, better than the supposed whole milk of four years ago.

Bread, too, which a few years ago was too often adulterated with alum, is now rarely so sophisticated. The evidence on this point is, however, somewhat defective. Before the passing of the Statute of 1872 no means, at the same time trustworthy and readily applicable, were known, by which the quantity of alum added to bread could be determined, and it is perhaps not too much to state that prior to the year 1873 the quantity of alum in a loaf was scarcely ever accurately given in an analytical report. That alum was habitually introduced into bread is a fact abundantly proved by other means than those furnished by chemical analysis. We know of at least one pharmaceutical chemist whose practice it was to supply "stuff" to bakers in a certain district of London, and whose course of trade was completely revolutionised by the Act of 1872. It is pretty certain that alum is now frequently added to bread, but usually in such small proportions that its detection is a matter of uncertainty and its effects upon health perhaps *nil*.

The Customs now under a clause of the Act of last Session inspects all tea before delivery; and all doubtful imports of that article are analysed in the laboratory of that Governmental Department. Immense good has thus resulted, and the worthless stuff which formerly passed under the name of tea, but which was in reality tea only in name, is now rarely met with.

A perusal of the works before us has convinced us that a

comprehensive, yet concise and trustworthy, manual of food and drug analysis is still a desideratum. The time to write such a work has perhaps scarcely yet arrived. The experience of public analysts is at present somewhat limited. The processes hitherto employed have been tentative, and in many cases fallacious. The rapid strides which have recently been made in chemical and microscopical science, and the stimulus imparted by the exigencies of the acts relative to adulteration will nevertheless, we doubt not, ere long result in the production of some trustworthy guide to food analysis from the hands of some competent chemist. Soubeiran's work is merely an inferior edition of Hassall's. Mr. Wanklyn's little volume on milk analysis is admirable in its way, and though we can speak less favorably of his 'Tea, Coffee, and Cocoa Analysis,' we must express a hope that he will give us more of his pithy treatises.

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#### IX.—The Mucous Membrane of the Uterus, the Decidua and Placenta.<sup>1</sup>

THE movement in the study of the physiology of the organs of generation in the female, begun in 1821, when Power enunciated the law of periodical and spontaneous discharge of ova, and continued by the researches of Girdwood, Jones, and Paterson in this country; of Negrier, Bischoff, Raciborski, Pouchet, and Coste, abroad, culminated about 1846 in the establishment of that law upon a firm basis. This movement was chiefly devoted to the study of the changes which take place in the ovary, and the uterus did not receive that attention which its importance demanded. During the twenty years that followed great progress was made in the study of the ovum, and though some remarkable changes were observed to occur in the uterus, yet the natural history of the organ remained comparatively unknown.

The papers before us form a contribution towards the elucidation of some obscure points in the history of the mucous membrane of the uterus. They trace its history from birth up to puberty, during sexual life and old age.

The changes which take place in this membrane during early life have attracted no attention and were quite unknown. Slight as they are, they are of considerable importance, inasmuch as they show

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<sup>1</sup> 1. *The Mucous Membrane of the Uterus.* By GEO. J. ENGELMANN, A.M., M.D. New York, 1876.

2. *Lectures on the Comparative Anatomy of the Placenta.* By WM. TURNER, M.B. (Lond.), Professor of Anatomy in the University of Edinburgh. (First series.) Edinburgh, 1876.

that during the period of infancy the uterus does not remain quiescent, as was generally supposed, until shortly before puberty. In the foetus the membrane lining the body of the uterus possesses no glands, and they become apparent only during the third or fourth year of life. Dr. Engelmann says—

“A change takes place in the third or fourth year, at which time the membrane has increased in thickness to 0·0118—0·0196 inch (0·3—0·5 mm.), and the first traces of the developing glands appear in the shape of small crypt-like depressions, either simple, or in clusters of two or three, with a common opening; a delicate epithelium lines these sinuses, for glands we cannot yet call them. From this time onward very few changes take place until the tenth year, when the womb develops more rapidly and approximates in shape that of the mature organ at puberty. The mucous membrane has attained a thickness of 0·0275—0·0315 inch (0·7—0·8 mm.), and the glands are more numerous and more completely developed, forming no longer shallow crypts, but straight ducts, 0·00098 inch (0·025 mm.) in diameter, extending to a greater depth than before, sometimes throughout the entire thickness of the mucosa and even to the muscular structure.”

From this time the membrane continues to develop until puberty. At puberty the uterus has attained its full size in the unimpregnated condition, and its mucous membrane becomes the seat of marked and important changes—changes which indicate womanhood and the capability of being impregnated.

It has been generally believed that the uterus remains in a state of inactivity during the intervals between successive menstrual flows, and that at or about the last-named periods only, the organ shows any sign of activity. Drs. Kundral and Engelmann assent to this view, but in a modified form. They believe that the period of activity near the time of the menstrual flow lasts longer than has been generally supposed, and that the periods of rest are consequently so much shorter. This view is open to serious objections, which we shall refer to when we come to discuss the periodical changes which take place in the organ. They say, “we rarely find a completely normal, inactive, uterine mucosa which seems to indicate that the actual period of rest for that membrane is much shorter than is generally supposed.” That these are periods of uterine rest cannot be doubted, but they are not to be looked for in the uterus of a healthy woman, who menstruates regularly and normally, during the period of sexual activity; they should be sought in the uteri of women suffering from chronic disease, and who have ceased to menstruate for some time. In many such cases the uterus appears healthy, but quite inactive. The truth of the latter statement is established by the fact of the absence of menstruation, and that the inner surface of such organs present the characters of inactivity, when

compared with the inner surface of the uteri of healthy women who menstruate regularly.

On examining such inactive uteri there is found on their inner surface a thin (about  $\frac{1}{16}$  inch) layer of soft, gray or pale tissue, which consists of a superficial layer of ciliated columnar epithelium, the tissue beneath being composed of roundish and fusiform cells, the round cells being more numerous near the surface, and the fusiform predominating in the deeper structure. This layer of soft tissue is in contact with the muscularis beneath without the interposition of any connective tissue between. There is in this inactive state usually a rather marked distinction between the muscularis and the mucosa, but it should be observed that the round cells of the soft tissue are found everywhere between the bundles of the muscular fibre cells forming the wall of the uterus, but always in decreasing quantity in the deeper layers of the wall. These cells are embedded in a structureless transparent matrix. In the mucous membrane are found besides vessels and nerves, numerous tubular glands opening on the surface. They run more or less perpendicularly from the surface to the muscular tissue, and are somewhat wavy; they occasionally bifurcate towards their lower ends. They are supposed to terminate on the surface of the muscularis by somewhat swollen ends, and since Weber first figured the fundi of the glands resting on the muscular wall, this view has generally been accepted. The glands do not, however, terminate in this manner, but enter into the muscularis, and terminate in that tissue in some as yet unknown way. The presence of glandular tissue in the situation just mentioned is put beyond question by the observations of several observers. The glands are lined by ciliated columnar epithelium. Williams maintains that the deeper portions are lined by round cells, and Turner has observed a similar condition in the uterus of the kangaroo.

We come now to examine the changes which take place in the mucous membrane from month to month—in fact during the menstrual period. It has been long known that the source of the menstrual blood is the body of the uterus. There is some difference of opinion, however, as to the part taken by the Fallopian tubes and ovaries in contributing to this hæmorrhage; for it has been observed in uteri of those dying during the catamenia that the tubes occasionally contain a certain amount of blood, and that hæmorrhage results as a consequence of rupture of the Graafian follicle. Whether the blood found in the tubes is poured out from the vessels of those structures, or whether it is due to regurgitation from the uterus, or to passage of blood into them from the ovary, has not yet been decided. In the majority of instances examined the tubes contained no blood, and when present it was in such small quantities that it may fairly be attributed, in the greater num-

ber of cases at least, to post-mortem changes. It is maintained by many that a certain quantity of blood is poured by the Graafian follicle into the Fallopian tubes and that this passes into the uterus; but, when it is remembered that in many cases the follicle has ruptured before menstruation, sometimes after it has ceased, and sometimes and not always during the catamenial flow, it cannot be maintained that, as a rule, the follicle contributes blood towards the menstrual discharge.

Various opinions have been held with regard to the means by which the blood escapes from the uterine vessels, and it is only within the last few years that data sufficient for the determination of this point have been obtained. It was at one time maintained that the blood escaped in the form of a secretion. This view was in vogue before it was known that the great part of the menstrual fluid consisted of pure blood. When it was discovered to what portion of the discharge the name of secretion could be applied with accuracy—then this view was discarded. Coste supposed the escape to take place by transudation through the capillaries of the mucous membrane of the uterus; and when it became known that the corpuscles of the blood could pass through the delicate walls of those tubes there was no great difficulty in receiving Coste's view. Indeed it is by no means improbable that early in the process such transudation may take place, but it is certain that it plays no important part in menstruation. It has been further suggested that the blood escaped by permanent vascular orifices. This is an hypothesis which has but little in its favour, and its supporters must have been driven hard to have recourse to such a purely fanciful theory. It is evident that those who believed it had mistaken the orifices of the utricular glands for orifices of blood-vessels, for the fact is undeniable that during menstruation blood may be made to ooze out of these orifices. The most commonly, perhaps the universally accepted view now is, that the blood escapes from the lacerated vessels of the mucous membrane of the body of the uterus. Dr. Arthur Farre had long ago observed this lacerated state of the vessels in the uteri of those who had died during menstruation, but he felt that he had not seen a sufficient number of examples to warrant him in stating it to be the real and constant cause of the menstrual hæmorrhage. To obtain a clear view of the manner of the hæmorrhage it is necessary first to study the preliminary changes which take place in the mucous membrane.

It has been long known that the mucous membrane of the body of the uterus becomes thicker, softer, and apparently more vascular towards the approach of the menstrual flow. This swelling and tumefaction has been hitherto regarded as a rapid process taking place suddenly towards the end of the inter-menstrual interval, under the influence of the ovarian stimulus. Drs. Kundral and Engel-

mann have found the mucous membrane swollen during the catamenia, but they maintain that the swelling lasts for a much longer period than is generally supposed. They say :

“In any case in which the ovary reveals a recently ruptured Graafian vesicle, or even a fully developed corpus luteum while the uterus and its appendages are tumefied and congested, the mucous membrane will be found in an essentially different form from that just described, *i. e.*, when the state is unquestionably that of menstruation and not of conception. The membrane is swollen to 0.118—0.236 inch (3.6 mm.) in thickness, of an almost pulpy consistence. Its surface is puffy, wavy, and in places reveals the delicately injected capillaries, after removal of its coating of whitish opaque mucus occasionally tinged with blood. In some cases the injection is more marked in certain parts of the membranes, while in others especially in those of sudden death, not due to hæmorrhage, it is intense throughout, giving it a uniformly red appearance.”

#### Again

“The microscopic changes which take place are equally marked, and prove the increase of the mucosa to be due to a tumefaction. I should perhaps more properly say hypertrophy of its superficial layers; its upper half in which the stroma appears less compact, is rich in embryonic cells, and has grown far above the original gland openings, circumvallating them, and thus causing the funnel-shaped depressions, those small pits which make the ostia seem enlarged. The tumefaction is owing to a proliferation of the round cells of the stroma and an enlargement of the individual cells of all kinds, including those of the glands themselves as well as an increase of the succulent, homogeneous, intercellular substance. A new formation of blood-vessels I have not been able to discover.”

Such are the changes preparatory to or rather which precede menstruation according to the authors of the papers before us. They ascribe the hæmorrhage not to congestion, but to fatty degeneration of the superficial layers of the mucous membrane involving not only the “cells of the inter-glandular tissue, but also the blood-vessels and the glandular and superficial epithelia.” The cause of this fatty degeneration Kundral thinks may be a deficiency of blood from want of newly formed blood-vessels. Evidence of the absence of formation of blood-vessels is however wanting. The detection of such structures would be very difficult. The tissue is of so peculiar a character, and the walls of the vessels of so delicate and transparent a nature, that it is often impossible to distinguish them under the microscope unless they have been naturally or artificially injected. When this is the case, however, as in the natural injection which precedes menstruation and which lasts after death, the vessels are so numerous and the mucous tissue so exceedingly vascular, that the absence of formation of new vessels cannot be admitted with-

out further evidence. Whatever be the cause of the fatty degeneration which takes place in the uterine mucous membrane, it probably plays a part much more important than that played by congestion in causing the hæmorrhage. They say—

"1. The congestion of the organ alone cannot cause the hæmorrhage, as we find a more marked hyperæmia, a greater turgor of vessels in the pregnant uterus and its mucosa, and yet no hæmorrhage follows.

"2. There are other physiological changes of the uterine mucosa in which a fatty degeneration of the tissues takes place, as in the gravid womb at term, or at any time previous when the ovum has perished and the tissues are thus prepared for its separation and expulsion, as in premature delivery and abortion.

"3. The hæmorrhage in the menstrual womb is confined to the surface of the lining membrane and the fatty degeneration is likewise more marked in its upper layer. I have never found extravasation of blood in the substance of the mucosa."

With reference to the above three propositions there can be no doubt in the mind of any one who has had opportunities of examining uteri during menstruation and pregnancy with regard to the first two. The third is open to question inasmuch as the extravasation of blood had been observed in the superficial though not into the deep layers of the mucous membrane.

As regards the changes which occur in the membrane during the period of hæmorrhage Kundral and Engelmann add:

"The destruction and detachment of a large part of the more exposed elements of the surface, and even of the glandular epithelium accompanies this process, in proof of which numerous epithelial cells, in a state of beginning fatty degeneration are found imbedded in the whitish somewhat bloody mucus which fills the uterine cavity. I cannot however agree to the assertion, that the entire epithelium of the surface is lost; until the beginning of the retrograde process it certainly remains entire and intact."

And again:

"The facts gathered warrant the conclusion that the mucous membrane of the womb begins to increase in thickness and succulence as the time of menstruation approaches, that this tumefaction is most marked during the period itself, and gradually decreases after the cessation of the catamenial discharge."

Such is the view of the monthly changes which take place in the uterus, enunciated in the papers before us. It differs materially in several important particulars from the view which has been maintained by some authors in this country and it becomes necessary to inquire on what data it is based? When we find at the beginning of the paper that the Pathological Institutes of Vienna and Berlin were the sources from which the material for the investigations was

obtained, our first impression was, that the material was abundant to superfluity, and when we came upon the following passage we were confirmed in that impression :

"My conclusions are based upon the examination of a large number of uteri in various conditions, and of ova as well as uteri during all the periods of gestation. Seventeen uteri were examined containing normal healthy ova in all stages of pregnancy, from the second week after conception to the full term ; of two hundred others some were virgin wombs, some exhibited the menstrual condition, some that after abortion, and others that from the first day to the sixth week after delivery ; in addition to these a large number were examined before the establishment of functional activity."

Here we find abundant material, as we shall see later on, for tracing the changes which take place in the mucous membrane during gestation. When we found that some of the uteri examined exhibited the menstrual condition we imagined that the organs of subjects whose menstrual history was known, and who had died at different periods of the catamenial flow from the first to the last day, had formed part of the specimens examined : on more careful attention, however, we found that the evidence of this condition was based upon the appearances presented by the uteri and ovaries, irrespective of the diseases of which the patients had died, and that in no case was the menstrual history accurately known. The nearest approach to such history of any of the unimpregnated uteri appears to be the following :

"In two of our cases, girls who died suddenly a few days before the catamenial period, the precise time of which could unfortunately not be ascertained."

"In other cases in which the catamenial discharge was said to have ceased several days before death, and in which well-developed corpora lutea were found . . . "

So that it appears that the material for investigation did not include a single uterus during the period of hæmorrhage.

Indeed Dr. Engelmann in the American version states such to be the case when he says : "I will cite both" (Dalton's), "as they cover a time of the catamenial period not observed by me : In the first in which death occurred during the menstrual period, an enlarged Graafian vesicle not yet ruptured was found in the ovary : in the second death had taken place at the termination of the period and the ovary revealed a Graafian vesicle, prominent and on the point of bursting." This want of accurate menstrual history in all the cases detracts greatly from the value of our author's observations on the condition of the uterus at the time referred to, and the absolute want of material for investigation of the changes during the menstrual flow renders their observations on that period merely hypotheses.

Another view of the monthly uterine changes is, that the mucous



membrane having become swollen, fatty degeneration sets in, which is followed by hæmorrhage with the superficial layers of the membrane, resulting in rapid disintegration and removal of the whole of the lining tissue. Pouchet had a theory that the mucous membrane was shed about the middle of the inter-menstrual interval. This was based on an examination of the discharges from the womb and not on anatomical observation. Pouchet's view has no relation to the view first stated by Dr. Tyler Smith that the mucous membrane of the body of the uterus was entirely removed during the menstrual flow. The chief objection made to this view is one based upon the present prevailing views on the genesis of tissue—namely, that if the whole of the mucous membrane be removed, it cannot be restored. To this it has been replied that a great portion of the muscular wall of the uterus is composed of muscularis mucosæ, and that therefore though that soft tissue which has been called the mucous membrane be removed, the tissue beneath is still mucosa but modified by great development of muscular fibre cells in its substance.

In favour of this view there are several observations. Almost if not all observers who have had opportunity of carefully examining the mucous membrane of the uterus during the catamenial flow agree in stating, that it is in a state of disintegration or dissolution. Dr. Arthur Farre, as has already been stated, had observed this; Dr. Tyler Smith says:

"I have had opportunity of examining several uteri taken from women who had died during the catamenial flow. In each of them I found the mucous membrane of the uterus either in a state of dissolution or entirely wanting. In one case, that of a woman previously in good health, who died suddenly from a fit of apoplexy whilst menstruating, the mucous membrane was entirely gone. At the upper part of the cervix uteri the break in the mucous membrane was very apparent. In the cervical canal the mucous membrane was perfect; but at the os uteri internum it ceased as abruptly as though it had been dissected away with a knife above this point. I had the assistance of Dr. Handfield Jones in examining this uterus with the microscope, and we could find no traces of the epithelium nor of the utricular glands."

Dr. Graily Hewitt has found the membrane in a state of disintegration in the uteri of those who had died during the menstrual flow. Dr. John Williams has described uteri of women who had died during the catamenia in which the membrane was found in various stages, from the stage of full development to that of complete removal. Dr. Underhill has described the uterus of a woman who died when the menstrual flow had almost ceased. He states that the superficial layer of the inner surface of the body was wanting and that the muscular fibre cells (amongst which glands were distin-

guished) reached to the surface. Such is the evidence in favour of the view that menstruation consists in the complete removal of a developed decidua. According to this view the decidua is developed by proliferation of the elements of the next layer of the muscular wall, which is regarded not as a muscle but as a modified mucous membrane—containing all the elements necessary for the production of a decidua: and that such decidua is gradually developed from the time when degeneration sets in into the effete decidua, that is just before a menstrual flow, until just before the next menstrual flow, when it has attained its full development and then becomes the subject of retrograde processes unless conception has taken place.

A question of considerable interest—that is the relation in time between ovulation and menstruation, and consequently the relation of conception to menstruation—is briefly discussed in the papers before us. The authors take opposite views on this point. Dr. Engelmann adheres to the view that the ovum which becomes impregnated was discharged in connection with the last appearing catamenia.

Dr. Engelmann says,

“The evidence we have of the simultaneous occurrence of these processes (menstruation and ovulation) appears to me conclusive, and it seems but natural that the high degree of congestion existing in the organs of generation should cause at the same time in the uterine mucosa tumefaction and hæmorrhage, and in the ovary ripening and rupture of the Graafian vesicle.

“I do not propose to say that the escape of the ovum takes place invariably upon one and the same day after hæmorrhage has set in, but the specimens examined by me do go far towards proving that the rupture of the Graafian follicle generally occurs towards the close of the catamenial period.

“I need only refer to cases already cited, two especially characteristic, known to have died shortly before the menstrual period, in which the mucous membrane already displayed the menstrual tumefaction, and no sign of retrograde metamorphosis; in these uteri menstrual hæmorrhage had not yet taken place, but was evidently soon to be expected, and no sign of a recently ruptured follicle was to be found in the ovaries.

“In the ovaries of those whose deaths had taken place during the continuance of the hæmorrhage clotted blood was found in the cavity of the follicle, indicative of recent rupture.

“Cases that had died shortly after the cessation of the catamenia showed the still tumefied somewhat disintegrated membrane and well-marked corpora lutea in the ovary.”

Then he quotes two cases from Dalton's *Essay on the 'Corpus Luteum'* (already cited), and a case recorded by Dr. Michel “of a woman executed on the second day of the catamenial flow, in whom the follicle was found filled with clotted blood, its rupture having

but very recently taken place, probably hastened by the circumstances of the case."

This evidence does not prove Dr. Engelmann's view. He seems to overlook the fact that menstruation now and then occurs without the occurrence of ovulation; while in the case recorded by Dr. Michel, as well as in the cases examined by Dr. Engelmann, in which death had taken place several days after the cessation of the catamenial flow, and in which well-marked corpora lutea were found, it is possible that the rupture of the follicle had taken place before the appearance of the catamenia. The two cases related by Dalton are strongly in favour of the view that conception takes place soon after menstruation, but their number is too small to generalize from, especially when we consider the evidence in favour of the opposite view, *i. e.* that conception takes place before menstruation, and that the ovum impregnated is discharged not in connection with the last appearing, but with the first absent catamenia.

The latter view has been maintained on clinical grounds by Löwenhardt, and Kundral supports it on anatomical grounds in the following words:

"On comparing the changes in the mucous membrane before the menstrual bleeding with those in a uterus in which an ovum in an early stage of development is found, we see, that the difference between the two is not qualitative, but quantitative only. This renders it possible that the growth and swelling of the mucous membrane during menstruation is only a preparation for a possible conception. During and after the bleeding the mucous membrane is in a state of retrograde metamorphosis; to assume in spite of this that a conception exists and that the already degenerated mucous membrane is excited to new growth, is less plausible. It is more natural likewise to assume that the escape of the egg from the follicle takes place before the bleeding, and that in the cases in which the conditions necessary for impregnation are present, bleeding does not take place. We can then recognise in the menstrual changes of the uterine mucous membrane a preparation for the reception of the ovum."

But this view of the relation of ovulation to menstruation is further supported by the observations of Reichert and Williams. Reichert, in twenty-three preparations bearing on this point, found four in which the Graafian follicle was not ruptured, the decidua menstrualis was more or less developed, and bleeding had not yet taken place. In one of the four the follicle was on the point of bursting. In eighteen hæmorrhage had taken place into the decidua menstrualis, and an egg had been discharged. In one an ovum had escaped but hæmorrhage had not begun. He concludes that the menstrual hæmorrhage probably takes place after rupture of

the Graafian follicle, and that the ovum impregnated is discharged in connection with the first absent catamenia.<sup>1</sup>

Clinical evidence is moreover in favour of this view. The history of the Jews, a prolific race, proves incontestably that conception takes place after the eighth day after the cessation of the catamenia—a fact which is quite incompatible with the theory that conception occurs generally during the first few days after menstruation. To learn the exact value of this clinical evidence there is a factor wanting—namely a nation amongst the members of which coitus is not practised except during the first eight days after the cessation of the catamenia. Were this forthcoming the question of the time of conception or at least of fruitful insemination would at once be settled.

At the same time that we maintain the general rule that conception and ovulation take place before the menstrual flow, still there are recorded examples where conception apparently followed a single coitus taking place soon after the cessation of the menses. Instances of this kind are however rare and must be regarded as exceptions to the rule; yet they show that intercourse taking place immediately after the cessation of a catamenial flow may occasionally prove fruitful.

*(To be continued.)*

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<sup>1</sup> Williams, after having examined sixteen preparations, concludes that in the great majority of cases, rupture of the Graafian follicle takes place before the hæmorrhage begins.

## Bibliographical Record.

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**Evolution of the Human Race from Apes.**<sup>1</sup>—This little book comprises two lectures “originally delivered to audiences of ladies and gentlemen in the Botanical Theatre of University College, London;” one in 1874, and the other after the lapse of a year in 1875. It is dedicated to Sir Robert Christison, and has an advertisement or first preface and a longer introduction added on. The lectures were intended to disabuse the minds “of some who have already been led unwittingly to accept the doctrine of evolution”—“a weak-minded class,” “who commit the absurdity of trying to reconcile the doctrine with belief in a personal *First Cause*.” In the treatment of the question before him the lecturer determined to “abstain from any theological discussion,” but this did not prevent him from writing as follows, in about ten lines afterwards:—“The belief in Revelation and a personal Creator is, no doubt, the alternative of the admission of evolution, but admission of evolution is not the necessary alternative of unbelief. Excluding all belief in Evolution and a personal Creator, for argument sake, I hold that the doctrine of evolution, unsanctioned, as I believe I show it to be by science, cannot be accepted as the alternative on any consideration, and must therefore be unconditionally and absolutely rejected.” This is a very powerful way of saying, that if any one believes in evolution he is an unbeliever, but that all unbelievers need not be evolutionists. Moreover, there is no scientific truth in the doctrine. Therefore an evolutionist is an unscientific infidel! The odium theologicum is particularly obnoxious to those who have had a medical training. It is contrary to the tone of thought and feeling which is inspired by the wide grasp of knowledge essential to the accomplished medical man; and it is offensive to the feelings of those who have lived long enough to remember the time when the term atheist was almost synonymous with medicus in the minds of the ignorant “good.”

Mr. Darwin, whatever may be thought of his opinions in biology, must be estimated by what he has written regarding the relation of evolution and theology. He wrote in his ‘*Origin of Species*,’

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<sup>1</sup> *Evolution of the Human Race from Apes; a Doctrine unsanctioned by Science.* By THOMAS WHEATON JONES, F.R.S., &c. 1876, pp. 69.

"There is a grandeur in this view of life, with its several powers having been originally breathed by the Creator into a few forms or into one; and that whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms most beautiful and most wonderful have been and are being evolved." This is not the language of an unscientific infidel. The accusation of atheism so freely scattered by Professor W. Jones shows that, although he taught Professor Huxley, and is always spoken of by that eminent thinker as his best teacher, he is still quite ignorant of the inner lives of the men who, he thinks, are disturbing and troubling the spiritual world. It is most indignantly repudiated by all the leaders of science, who, moreover, profess to see the majesty of the Creator in much more sublime and magnificent development than is within the conception of their accusers.

When, therefore, the author of these lectures to *ladies and gentlemen* talks about the theory of evolution "sapping the foundations of religion" he takes advantage of the ignorance and fanaticism of his audience. The scientific arguments against the theories of Lamarck, Darwin, and the evolutionists, can be properly stated and ought so to be, by men in Professor W. Jones's position, but, singularly enough, they never are. The weak points have been carefully pointed out by the candid Mr. Darwin, and his hypothesis has been accepted by some of the most able men of the day only as "a good working theory." Why does not Professor W. Jones attack these chinks in the armour of his enemies instead of scolding and misstating? If his book is to convert the weak-minded, possibly this may be a consistent but certainly not a judicious course; for the incipient Darwinite will only laugh when he reads "mankind are merely the sons and daughters of apes," and is told that this is the faith of his teachers. In no part of his first lecture does the author attempt to grapple with evolutionist theories scientifically, and he mistakes some of the arguments of his opponents. There is nothing more striking to old and new thinkers than the facts about rudimentary structures and organs. Take a well-known instance or two. The ox has no incisor teeth in the upper jaw, but its calf has; they are large, well developed, supplied with vessels and nerves, but they never come through the gum, are never used, and finally they are absorbed. Again, the Indian Rhinocerides have well developed front teeth, but the African species never has them sufficiently developed to come down and be useful. Man has an appendix vermiformis, which is rather a cause of accident than a fully developed structure. Are these freaks of nature, or is there any scheme in nature which can explain them? The evolutionist says that these rudimentary structures are the relics of what were useful organs in the ancestors of the species, and disbelieves in "freaks." The

anomaly in nature is with him a link in the chain of evidence. The earliest ruminants had incisors in both jaws, and the rudimentary upper teeth of the present species probably have a reference to that ancestral fact. This line of argument will apply to all such structures, and it must be admitted with regard to man's appendix that it is a relic of a past ancestry. One would have been glad if Professor Jones had just taken up these facts and hypotheses, and had handled them like a scientific man. In one sentence of these lectures every one will coincide:—"In the natural history of an organism we recognise something more than the manifestation of the physical and vital forces—physical forces as the attribute of the machinery, and vital forces as the mainspring of its action. We recognise in the aggregate plan a Divine idea, and in the fulfilment of its purpose an Almighty hand." Every evolutionist will admit all this except the distinctness of physical and vital force. But Professor W. Jones proceeds to say—"Evolutionists, as before stated, exclude the idea of a Creator, and attribute it all to chance, which is really the meaning of natural selection." We pass over some pages of the "lectures," hoping for light and some elaborate theory which will set men's minds at rest, but in vain; and the ladies and gentlemen are finally quieted by the assertion that "The question of the origin of man, it must be concluded, is one entirely beyond the pale of natural science."

In the second lecture Professor W. Jones confines himself in his disproof of the theory of evolution to an attack upon Haeckel's notions of the subject, and he evidently believes that all that this naturalist writes is the foundation and the very belief of the so-called advanced school. There is no man about whom there are such diverse opinions amongst those who are qualified to express an opinion on philosophical biology as the author of 'The History of Creation according to Natural Laws.' He is the subject of infinitely greater criticism than was Mr. Darwin, and for the simple reason that, unlike the English biologist, his facts are often discredited and the wildness of his hypotheses also. There has always been some reason and consecutive argument in Mr. Darwin, whose exactitude is, moreover, wonderful; but Haeckel—the delight of those who believe German work and German thought to be supreme—has met with sufficiently rough treatment at the hands of some of his fellow-countrymen, both as regards some of his so-called facts and his wondrous conclusion-jumping. There are two theories of life before the world—that of a supernatural special creation at many consecutive epochs in the world's history, and that of one outspringing of life in the early days of the globe, the continuance of life having been attended by variation in form. The second theory embraces the idea of law, and law is man's notion of the manner in which the lawgiver acts. It involves the idea of a first cause and

of a Creator, even if the theorist were a Turk, and it does not militate against the belief of the Christian. Is the first of these theories true and compatible with what we can glean from nature? Now, Professor W. Jones should have advanced his proofs in its favour, but he does not do this; he leaves us without a theory, and seeks to demolish evolution run mad, or Haeckelism. But he even fails to do this. Take, for instance, his management of the very knotty point of the development through a succession of significant changes of the mammalian embryo. He admits that the embryonic development of man is similar to that of other mammals, and that the embryonic development of these last has its resemblances with those of the lower vertebrata; and then denies the value of the facts to the evolutionist. Moreover, he gives his enemies a little assistance by introducing a hobby relating to the proper positions of the dorsal and ventral aspects of the vertebrata and invertebrata, showing that the two great groups are not so sharply defined after all. Then, strangely, he insists upon the invalidity of the well-known arguments about the vertebrate succession from the ascidian larva. Professor A. Jones is unfortunate in selecting Haeckel when attacking what is termed differentiation, and he denies that there is any such process in nature. His own views of what occurs during the development of structureless vitalized matter and his misconceptions of the belief of nine naturalists out of ten about it may be understood by carefully reading the following extraordinary statement:

“When by virtue of their original intrinsic qualities differently composed and endowed cells are under appropriate conditions developed into tissues, for example, each kind in its own proper way, we may truly call the process one of differentiation; but there is certainly no such process if by the expression is meant the development of different kinds of tissues from one and the same kinds of cells by their own desires and efforts. The reality of such a process is, in fact, the very thing to be contested as the false foundation on which the superstructure of evolution has been in a great measure built.”

If Professor W. Jones believes that the very distinguished biologists who teach evolution, and who do not necessarily credit Haeckelism, imagine that a piece of protoplasm can by any “desire and effort” produce granules in its substance and a vacuole and encircle the whole with a cell-wall, he is enormously deceived. Desire and effort are absurd terms in this case, and are crazy exaggerations of the proper term potentiality, or rather of Professor W. Jones’s own word, “intrinsic quality.” Towards the close of his lecture, which we must say was rather beyond the comprehension of ladies and gentlemen, the author gives a good review of Haeckel’s nonsense about our amphibian forefathers and the succession of forms of the “line” of man. Considering that the “line” of several of the



# **Biographical Record.**

Why are some distinct, and that there are huge  
 between man and the highest apes, and the  
 Who can say, for instance, what can have been  
 the progenitors of the lower American mon-  
 some they the great apes lose their air-sacs  
 to what process did he blow out in and amongst the  
 the subject again he will  
 the subject he is an orna-  
 of which he is an orna-  
 attempt it, let him dismiss  
 arguments, and it may be  
 will not, for the sake of  
 a philosophical evolution

## **Zoology for Students.**

Any one who writes a book on zoology  
 prepared for adverse criticism, for it is  
 the professors of  
 Moreover, if the author adheres  
 to be unfavorably noticed by comparative  
 deal too much with visceral anatomy  
 the occasional parties of logical  
 and outside in classification. A  
 evolutionists, final  
 old-fashioned  
 so that the modern  
 Dr.  
 plunges into the  
 writes, "In  
 the teaching  
 of the science in Eng-  
 comparative anatomists,  
 somewhat of the traditions of Hunter and  
 small mercies from all the  
 although there is a vast  
 a short preface from  
 with a distinctiveness, and

might be able to transmit to a future generation, in the days of a  
 retrograde anatomy, somewhat of the traditions of Hunter and  
 Owen." Of course he has received small mercies from all the  
 amount of information in the book, and of the kind we should  
 expect from such a student of nature as Dr. Carter Blake.  
 He has been very fortunate in obtaining a short preface from  
 Prof. Owen, thus stamping the whole work with a distinctiveness, and

*Zoology for Students.* By C. CARTER BLAKE. With a preface by RICHARD  
 OWEN, C.B., F.R.S., &c. Pp. 98. 1876.

making it the hand-book of those who think with that great biologist rather than with the so-called advanced school whom Dr. Blake taunts with retrogression.

The preface is delightfully Owenic, and treats sternly and with a grim humour the doctrine of vertebrate classification by the cerebral developments. Happy in its consecutiveness, amusing in its paradoxes, and rather grandiose in its style, it strikes fiercely at all opponents, without deigning to mention their names or even opinions. Owen of course insists upon the primary importance of the development of the nervous centres in any classification, and upon the subsidiary value of "vascular, respiratory, locomotive, tegumentary, developmental structures." The rise of the nervous system, and especially of the thinking part, produces by action on the muscular fibres waste, and "necessitates corresponding improvements in the machinery for renovation." The heart is adapted, and the blood, by its greater number of coloured discs, is made fitter, for renewal of the waste, the highest perfection being attained by the heart, which circulates perfectly pure blood, and not a mixed arterial and venous fluid. All this produces a rise in the temperature of the body, and vertebrates are divided into those with "wraps" and with "no wraps." Vertebrata inferior to birds have no "wraps," and their temperature rises and falls with the surrounding medium, but those with feathers and furs enjoy an unchanging higher temperature. Wonderful are the connections between the body and mind, said the ancients; and now we say wonderful are those between a comfortable body and a light-hearted mind. According to Owen, in reasoning out his conclusions on the above, a good balance at one's banker's should elevate the intellect, and there is comfort in store for rich Philistines. He writes, "The rise in the faculty of forming ideas and enjoying sensations evokes the faculty of expression, such as is exemplified in the varied songs of birds and the manifold cackle or vocal utterances of the poultry yard." Reminded of what is called the "Cock-a-doodle" theory of the evolution of birds from Reptilia, Owen proceeds to snub it. "The contrast which the crocodile, highest of existing 'cold-blooded' vertebrates, presents in its poor brain, mixed circulation, slow breathing, low dependent temperature, hard naked tegument, habitual muteness rarely broken by hoarse monotonous bellowing, opposes its association in the same class with the bird. Eagles and crocodiles are both, indeed, oviparous, but spiders and slugs also lay eggs. Man has a double occipital condyle, and so, likewise, has the frog." In passing on, after expressing his dissatisfaction with a classification which depends on placental structures, Owen considers the brain as "the taxonomist's unexceptional guide," and refers to his well-known division of the Vertebrata into Lyencephala, Lissencephala, Gyrencephala, and Archencephala, insisting that most

of the varieties of placentation are to be found in the second and third groups. Dr. Carter Blake follows these precepts and no others, and therefore it is only fair to consider his book from this standpoint. He is, of course, responsible for facts, and especially as he has gone out of his way to give the geographical and geological distribution of many animals. In his first chapter on man there is a succinct summing up of the anatomical distinctions between man and apes, which is very good and to the point; it might have contained the fact that gorillas and most apes have large laryngeal pouches, and that the microscopical characters of the brain are coarser than those of man. There is a good diagram given of the foot of a gorilla, showing that it is a foot with a thumb-toe; and also a useful scheme of the genera of monkeys.

In the description of the Equidea a little "retrograde anatomy" crops up, showing that Dr. Carter Blake has been flirting with the evolutionists. He notices, "A link was thus formed between hipparion and the tapirs on one side and the horse on the other, and a scheme exhibited showing by what means a process of gradual development might arise which would convert one generic form into the other." Some very interesting details are given (p. 37) regarding the antiquity of the domestic horse, and the author seems disposed to consider the horse, which was domesticated in Britain before the Latin civilization, to be a descendant of the *Equus fossilis* of the cave and river gravel periods. A good notice on the elephant in a zoologico-geological sense follows, and then necessarily short chapters on the Sirenæ and Cetacea. Placing Galeopithecus amongst the bats, to which he assigns an order, he passes on to consider the rest of the Lissencephala. He will be opposed by palæontologists in his assertion that the Purbeck mammalia, Spalacotherium and Triconodon, are smooth-brained (placental) insectivora, as the evidence is in favour of their being marsupial implantationals. There is a good chapter on the Edentata, much better than that in most books of the kind, and such a description of the Marsupials as one could give to a class in an ordinary course of lectures; but, oddly enough, the inflexion of the lower jaw, almost a characteristic, is omitted. The birds occupy much space, and then the Reptilia afford the author opportunities for receiving very adverse criticism, for he has gone into palæontology to the detriment of his zoology. His omission to associate Huxley's name with Cope's in the matter of the connection between the struthiose birds and the Dinosauria is not ingenuous. Some important misprints occur here; thus, it is put in that the Anura *have no* metamorphosis, the meaning being *undergo* instead of "have no," and Dr. Carter Blake will have to supervise his last revise very carefully for the next edition.

The Invertebrata are treated of in several chapters, and on the whole this section does come up to the level of the first part of the

book. It would be wonderful, indeed, if it did, for the progress of science adds so much year by year to the histology and development of every order of Invertebrata, that it is impossible for any one to keep the pace. It appears to be a rule to commence works on zoology with the lowest form of life, and then they of course receive great attention, and to the detriment of the Vertebrata; but in this instance the inevitable want of space has crowded together at the end of the book the history of the Invertebrata. In the next edition Dr. C. Blake will have to add much that is new in the "Radiata," especially in the Echinodermata and Actinozoa, and his attention will be directed to late researches on the Infusoria. But it must be admitted that if Dr. C. Blake can give a course of lectures to students which will contain all that is in his book they are lucky, and may carry away a vast amount of knowledge, tinged though it be with Owenism rather than with the fashionable theories of the day. What students really require is a good book on zoology which shall not have so much "inside anatomy" and palæontology in it, and we would direct Dr. Carter Blake's attention to this opinion. He is a hard-working student of nature, an anthropologist of reputation, and a very able human osteologist, and with these qualifications he has a fair field open before him.

**Pelvic Measurements.**<sup>1</sup>—The subject of pelvic measurements is once more cropping up for discussion amongst anatomists. Since the publication of the classical works by G. and W. Vrolik, M. J. Weber, Zassiger, Martin, Van West, Joulin, Pruner Bey, and Huxley, we have had some important and suggestive notes which are contained in the ninety-seventh and ninety-eighth pages of the newly issued supplement to Barnard Davis's 'Thesaurus Craniorum,' which insist justly on the fact that, up to the present time, observers have contradicted one another as to the distinguishing features of the male and female pelvis. Humphry had well summarised the known facts ('Skeleton,' p. 444) when he said "the pelvis of the female is altogether a lighter, more expanded, and less compact structure than that of the male. Its processes are shorter and further apart, its cavity shallower and wider, and the several diameters are somewhat greater, particularly the transverse diameter of the basin, which measures 5·4 inches, that of the male being only 5·1 inches." On this Dr. Barnard Davis has noted that even these measurements cannot be taken as absolute; they are only individual, not universal. Thus we see that in the skeleton of an Aino woman from the island of Jesso the transverse diameter of the brim only reached 4·7 inches while in a male Illinois Indian it extended to five inches. Dr. Barnard Davis considers that the angle formed by the pubic bones may be taken almost as an infallible indication of the male or

<sup>1</sup> 1. *Le Bassin dans les sexes et dans les races.* Par R. VERNEAU, M.D. Paris. Paris and London.

female. In an Aino woman it is  $90^{\circ}$ , in an Australian female  $90^{\circ}$ ; these dimensions being exceeded in a Moriori from New Zealand and in a Hottentot, both females, whilst in no male of his collection does it extend much beyond  $80^{\circ}$ . Humphry has explained that this flatness of the pubic arch is always accompanied with lightness, thinness, and shallowness.

We have now an important work by Dr. Verneau, which comprises the result of the examination of 208 adult pelves of various races and different sexes. His work is divided into four distinct parts. In the first he gives an abridged history of the subject, with a bibliographical index. In the second he describes the pelvis in the European, laying especial stress on the brim. It is necessary before arriving at any general ethnical classificatory results to have a standard to which reference can be made in all cases. This term of comparison can only be the European pelvis, and for this reason it has been described with some detail. In the third part the author searches for the various sexual differences which may be recognised in European pelves; and the fourth is devoted to the study of the differences which are presented in the pelves of various races of man. The author, as is usual with Frenchmen, expresses all his results in millimètres.

The author well points out that Prichard, in his 'Natural History of the Races of Man,' was content to examine the conclusions of Camper, Weber, Vrolik, and Soemmering, without bringing a single new document before the imperfectly educated public who perused his compilation, and which even at that time were at a lower level than in France or Germany. Dr. Joulin made it appear that the examination of the pelvis did not lead to the establishment of more than two great divisions in the human race, and received the well-grounded censure of Dr. Barnard Davis, who could not attach all the importance desirable to a work in which the races of New Guinea, of Madagascar, of the coast of Mozambique and of the eastern coast of Africa, are confounded together under the common term of Negroes, and in which the Bushmen and Peruvians are amalgamated under the hypothetical denomination of Mongols. It is certainly difficult to find differences among races of men, when one race alone is made to comprise races as distinct as those above mentioned. Herr Carl Martin's measurements were, like those of Dr. Arthur Farre, destined for purely obstetrical purposes, but, unlike them, have that occasional inaccuracy which is not uncommon in German works which parade detail as their especial feature. Dr. Fritsch's work on the populations of South Africa contains much valuable information on the pelves of Caffres, Hottentots, and Bosjesmen. Many of the above-named writers occupied themselves too exclusively with the female pelvis, but Dr. Verneau devotes his attention to the male pelvis as well.

With regard to his description of the pelvis, it will be found to agree with the tone of thought of most French writers and with that of Mr. Thomas Cooke, who has brilliantly applied the French method of analysis of structure to English anatomical teaching.

The third part, on the sexual differences which are presented by the pelvis, is of more importance. These differences are much more salient than those of any other part of the skeleton. They are of two orders—firstly, those which are analogous to those found in other regions, and which are derived from marks imprinted on the bones by the muscular system, and by which the male exhibits a rougher surface with higher elevations and deeper depressions. The second category of differences relates to the presence in the female of a uterus intermediate to the bladder and rectum. The presence of this organ is sufficient to explain the characters which are met with in the female lower pelvis. Some of the differential characters which are constantly referred to are, according to Dr. Verneau, of no value. Such are the form of the ischio-pubic foramen. Some are entirely erroneous, and he cites the allegation that the concavity of the sacrum is greater in the female, as an example. He offers as decisive characters the facts that the pubic spines are more divaricated (*écartées*), that the iliac fossæ are flatter, and that all the diameters are smaller. One exception may be given to the latter strong statement, viz. that of the greater distance of the antero-superior iliac spine from the point of the sacro-iliac articulation which corresponds to the upper outlet.

It is clear, according to Dr. Verneau, that the pelvis presents in females a peculiar aspect, tending towards the *évasement* of the lower portion and the diminution of the height.

The differences are marked almost exclusively on the lower pelvis, and are determined by the presence of the uterus.

The form of the upper circumference is the same in both sexes; the relation of the maximum antero-posterior diameter to the transverse diameter is 0.62 in both. All the dimensions of the internal iliac fossa are less in the female, with the exception of the distance which separates the antero-superior iliac spine from the sacro-iliac articulation. The internal iliac fossa is less excavated in females. The dorsal part, that is to say the iliac tuberosity, is much more developed and produced more behind than in men. The pubic spines are further apart in females. In these all the diameters of the upper outlet are greater than in men; this difference is especially visible as regards the transverse diameter. The upper outlet presents in females a more rounded condition, which partly tends to the increase of the maximum transverse diameter, and partly to the position of this diameter, which is more forwardly situated than in men. The large sciatic notch is less open and deeper in man. In him also the top of the sciatic spines is sometimes inside the postero-inferior

iliac spines; in females it is always outside. In a male pelvis the distance which separates the sciatic spines is rarely more than 107 millimètres, and is often below ninety millimètres. In the female the same interval often surpasses 107, and is never below ninety millimètres. The maximum transverse diameter of the lower outlet in females is nearly fifteen millimètres more than that in man. The pubic arch is more open in the female pelvis ( $75^{\circ}$  in the female, and  $58^{\circ}$  in the male). The summit of the above angle is always rounded in the first. The ischio-pubic tubercle is more outwardly directed. In the female the sacrum and the coccyx are less elevated and more flattened. The cotyloid cavity is smaller, and inclined less backwardly and forwardly. The distance of the two cotyloid cavities is greater when measured inwardly, and smaller when measured from the ilio-sciatic notches. The sub-pubic foramen is not, as has been erroneously said, oval in men and triangular in females. It is relatively larger in the latter, and more oblique outwardly and inferiorly. The distance between the ischia is greater in the female sex. All the vertical diameters are greater than in man. The total height of the pelvis attains in man 220 millimètres, but in females only 197 millimètres. In the male pelvis the distance from the sciatic spine to the most elevated spot on the iliac spine is on an average 167 millimètres, in the female pelvis it only attains about 150 millimètres. The distance from the sciatic spine is on an average in females 137 millimètres, in man this average reaches 150 millimètres. The interval between the antero-posterior iliac spine and the lower part of the ischium being 165 millimètres in the female, is on an average 182 millimètres in the male pelvis. The maximum in the female never attains the last-named figure. The relations between the maximum vertical diameter and the maximum transverse diameter is in the female only 0.74, whilst in man it surpasses 0.79.

With regard to the examination of the shape of the pelvis according to race, we feel that we must pass briefly over this part of the subject, which rather appertains to the province of the anthropologist than to that of the teacher of medical anatomy. Dr. Verneau describes carefully the pelvises of several races, and points out marks of distinction which may serve in other hands to render less probable and conclusive the broad distinctions of sex which he has drawn, taking the European as a type specimen. It is obvious that a surgeon who *e.g.* has to diagnose in a case regarding the identity of a pelvis in Australia, China, or South America, has before him the possibility of error if he has merely studied the rough canons of differentiation which are taught in the school of anatomy of England. Yet the rules which Dr. Verneau had so clearly laid down appear not to be invalidated by the further anatomy of the pelvis according to race. A careful examination is given of the pelvis in numerous races. From this examination the author hopes

that the comparison of pelvic form will be as convenient in time as that of the skull, and will form the foundation one day of the classification of the races of man. On this we have little to say, excepting that there is nothing in the form of the pelvis which would make it less worthy a note whereby to classify as dissimilar and various races than are presented by the texture of the hair or the language. And now that a few scientific men identify Egyptians and Australians, Coreans and Ashantee Negroes, and Easter Islanders with every other race (including those of Thrace) which enlarged the ear-lobes, there is no limit to which the misapplication of human ingenuity may not be expected. Dr. Verneau has, however, placed a number of facts before us in his large and costly volume, with its sixteen plates, for which we thank him.

**Diseases of Modern Life.**<sup>1</sup>—When a second edition of a work of this kind is called for within six months of its first publication it may fairly be assumed that it is adapted to the views and wants of those to whom it is addressed; and this being so, the task of the critic becomes wellnigh a work of supererogation. With this persuasion, therefore, we shall accord the treatise only a brief notice. But apart from the above consideration, the book would not call for a lengthened review in a medical journal, for it is especially addressed to the “intelligent public,” and consequently has not for its prime purpose the development of truths or the enunciation of facts new to the medical profession. It rather seeks to place before non-medical readers a selection of facts generally admitted, with the view of awakening their attention to the laws of health and to the most important causes of disease, so that they may learn to obey the former and to avoid the latter. And assuredly the author has admirably used his opportunity and fulfilled his purpose, by forcibly depicting the causes of disease, making clear to his readers the consequences that must ensue on rebellion against the necessary conditions of health.

Indeed, in the ardour of his pursuit it must be admitted that he speaks too much like an oracle, whose conclusions are incontrovertible or infallible; that he dogmatizes too vehemently, and that hypotheses are made to serve in the place of verified conclusions. Nevertheless an excuse may be found for all this. He is addressing readers who have not the opportunity of examining for themselves; who must be taught by a master, and are willing to sit at the feet of one, and consequently desire to hear no uncertain sound. Moreover, if good is to be effected, if the public attention is to be aroused to the fatal consequences attendant upon luxury and vicious indul-

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<sup>1</sup> *Diseases of Modern Life.* By B. W. RICHARDSON, M.D., F.R.S. Second edition. London, 1876.



gences, the warning voice must be as the sound of a trumpet, with no faltering notes.

The title of the work is, it must be granted, a taking one for the public, but to its fitness as indicating the subject-matter exception may be taken. It possesses a sweet ambiguity; it leaves it open to surmise whether the author is to write about diseases prevailing at the present day, or only about such as are peculiar to it and new, and induced by the habits, vices, and employments of the existing generation. However, we have it soon made clear to us that Dr. Richardson has no new diseases, or such as are peculiar to our times, to write about, for he says (at p. 15)—

"In brief, the more deeply we study the past history of medicine the surer is the conviction that, throughout the whole of the known period of human existence on the earth, not one new disease has been added, not one withdrawn;" *ergo*, there are no diseases peculiar to modern life.

On the other hand, it is not within the scope of his treatise to write about diseases in general prevalent in "modern life," for that would be tantamount to discoursing at large on general and special pathology. And, indeed, when we examine the chapters of the volume we see at once that no such grandiose scheme has presented itself to the author; but rather the more modest and useful one of placing before his readers, in perspicuous and very comprehensible language, an outline of the causes of our commonest diseases, with particular reference to those causes which luxurious and evil habits, over-work, and unhealthy employments, engender amongst us.

But these causes, almost without exception, are not new or peculiar to modern life; and the dictum, that there is no new disease known in the history of mankind, does not surpass in comprehensiveness the assertion we make as to causes of disease. We have hitherto dwelt on what the book does not deal with; it will be advantageous to state what it does contain, but, for reasons already given, this statement will not comprise an analysis of the subject-matter, but simply an enumeration of the principal contents.

The author's opening chapter is, on "natural life to natural death—euthanasia," but its chief reference is to death by "natural decay," a result due, in his opinion, to the ultimate triumph of "gravitation" over vital or physiological action. For our own part, we do not quite apprehend this fact, not understanding how and why the simple mechanical force of gravitation becomes the subvertor of all living processes, and the consequent prime factor in producing disease and death. Dr. Richardson, moreover, refers to death by old age as euthanasia, and as a natural physical process akin to birth, but, in an after section of this first part of the book, has a chapter on the "phenomena of disease incidental to old age and natural decay," which, when read in connection with the first chapter, somewhat

modifies the impression there given, and detracts from the pretty picture of the old man's exit from the world and the welcome euthanasia.

The other chapters in "Part First" are:—on "the phenomena of disease; classification and distribution;" "disease antecedent to birth," a chapter well calculated to stir up the curiosity of the general public; "external origins and causes of disease;" "phenomena of disease from causes external and uncontrollable," and "phenomena of disease from causes external and communicable." This Part First is, therefore, occupied with the consideration of the general etiology of disease. "Part the Second" deals with particular causes of disease, indicates their mode of action, and suggests means for obviating it. Amongst these special causes the author includes some which, if not new to the world, are probably more active and more widely diffused in the present than in past days; so far, therefore, he may claim to fulfil the design of his treatise in harmony with the title.

In this portion of the work we have reproduced much of what Dr. Richardson has previously written—and well written—in a series of essays on excessive mental work and worry, on excessive physical strain, and on the abuse of alcohol; and in the chapters devoted to these and other subjects, such as the use of narcotics—*e.g.* tobacco—late hours, errors of dress, &c., reads the public some valuable lessons that ought to be taken to heart. A critical medical reader, however, cannot go to the full length with him in all that he avers—in all the conclusions and lessons he advances. In his efforts to be pictorial and incisive, to round off his sentences well, and to be comprehensive in his portraiture and summaries of phenomena, and of the courses and consequences of disorders, he is, at times, betrayed into doubtful assertions and hazardous conclusions. His strong assertions respecting the use of alcohol have been subjected to criticism in the first article of this present number of the 'Review,' and his counterblast to tobacco is open to animadversion on the part of those well versed in the uses and abuses of "the weed." Indeed, in reference to the terrific picture he draws of the state of the stomach, implied as produced in its interior when the learner to smoke makes his often painful first efforts, it becomes us to protest against it as a portraiture greatly exaggerated, and such as should not be depicted as conveying a general truth to the non-professional public, based on actual physiological knowledge.

It is an ungracious and unwelcome task to point out defects in a work of so much value as an instructor of the public in what relates to its best interests, as well its sanitary as its social and moral; we shall, therefore, forbear singling out any other remarks of the author for criticism, having much greater satisfaction in commending his work to our medical readers as well as to the

"intelligent public," being fully persuaded that all may gather from it useful information and lessons of value, and that if society will act up to its precepts the world will be all the better for its publication.

**Nicholson's Text-Book of Zoology.**<sup>1</sup>—The character of Professor Nicholson's work in promoting the study of natural history is thoroughly well known and appreciated; and, as a matter of course, we receive the marks of appreciation in the form of new editions called for by the public. It is enough to make known to our readers the appearance of a second edition to this first of the series of three text-books on zoology, by which he leads his pupils onwards in the science of animal forms, organization, and modes of existence. The only point we would remark upon is that the present edition is "revised and considerably enlarged." Now, revision is a necessity by reason of the advance constantly made in science, but the enlargement of each new edition is neither necessary nor an advantage. A perpetual growth in size is an evil in an introductory treatise. If new discoveries necessitate new paragraphs, they will also render invalid or incorrect other paragraphs, and these should be excised, and the new matter be inwrought and not merely added to.

**Fagge's Catalogue of Skin Diseases.**<sup>2</sup>—As a pathological museum that of Guy's Hospital is not equalled in this kingdom for the number, value, and rarity of its specimens. The catalogue now before us shows how rich it is in models exhibiting the numerous forms and varieties of skin diseases, and consequently how admirable a field for the study of those diseases is therein offered; one indeed only second to that afforded in the living subject, and superior in teaching power to any collection of plates. In fact, from the attentive examination of these models the eye can take in and infix on the mind, within a short period, a more accurate and lasting conception of the diseases illustrated, than can be gained by a prolonged course of reading of the most accurately and well-described features of those maladies. At the same time the looker on wants an interpreter, and a most efficient one is found in Dr. Hilton Fagge, who has catalogued the specimens and made them tell their own tale. The term catalogue applied to this volume by no means conveys a proper idea of its character. It is not a dry, uninteresting list of the barbarous and semi-barbarous Greek and Latin derivatives, by which skin doctors apparently delight to designate the various rashes and discolorations that deform the surface of the

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<sup>1</sup> *An Introductory Text-Book of Zoology for the use of Junior Classes.* By H. ALLEYNE NICHOLSON, M.D., &c. Second Edition, revised and considerably enlarged. Edinburgh, 1876.

<sup>2</sup> *Catalogue of the Models of Diseases of the Skin in the Museum of Guy's Hospital.* By C. HILTON FAGGE, M.D. London, 1876. Pp. 269.

body, but a descriptive account of the special features of disease which each model is designed to represent, coupled with notes on the pathology. Moreover, to further elucidate both the specimen and the character of the disease it represents, a case is often recorded, in which is noted the course of the malady, the history of the patient and the treatment adopted, with the result.

The numerous additions made of late years to the collection, and the novelties introduced into the nomenclature of skin maladies, has rendered it necessary to re-number the models. To maintain, as it were, the continuity of the present with past catalogues, Dr. Fagge has given a table setting forth the former and the present numbers and names. By means of this table, therefore, references which have, in various previous publications, been made by numbers to the specimens in the Guy's museum can still be confirmed or checked; and where a new name has been given its equivalent in the old nomenclature is at once made apparent.

The artistic merit of the models is due to Mr. Towne; and, as Dr. Fagge remarks, "it is, indeed, a remarkable and most fortunate circumstance that, during the whole of a period of at least forty years, the models of skin diseases for our museum should have been made by a single artist, the same who supplied our splendid series of anatomical models."

Some very just and practical observations are made on the subject of the classification of skin diseases. Hebra's system is considered the best yet offered, but Dr. Fagge believes that it will not maintain itself for any great length of time. He gives his reasons for this opinion, and proceeds to express a conviction that a natural and perfect classification of skin diseases is not to be looked for, if we take a comprehensive view of them "as merely forming one branch of general pathology. It is not possible to classify the affections of any other organs. . . . Indeed there are not wanting good reasons why cutaneous eruptions should be more difficult to classify than the recognised diseases to which the different internal organs are liable." Nevertheless as a grouping of some sort is essential in constructing a catalogue, Dr. Fagge employs the following as a tolerably natural one and as reposing upon a pathological basis.

1. Inflammatory (or simply congestive) diseases.
2. Non-inflammatory diseases not having destructive tendencies.
3. Non-inflammatory diseases affecting the tissues of the skin profoundly, and generally destructive in their tendencies.
4. Affections of the appendages of the skin; and
5. Parasitic diseases.

We must send our readers to consult the book itself for a knowledge of the subordinate groups and the individual diseases included in them; promising them that, even without the great advantages to be derived from examination of the models themselves, they

cannot refer to this catalogue without deriving information. To those of them, who, with the book in hand, can study the objects described, a most perfect introduction and guide to the knowledge of cutaneous diseases will be in their possession.

**Quain's Anatomy.**<sup>1</sup>—If diligent continued labour coupled with judgment and knowledge, applied to any human work, can bring that work to perfection, Quain's Anatomy should be an instance of such a happy result. Fortunately the material wrought upon has not the limited and exhaustible nature of that submitted to the labour and genius of the sculptor, otherwise the repeated improvements in the shape of new editions would have sadly reduced the mass and be threatening to its entire annihilation; but on the contrary, it has been of a plastic nature, with powers of recuperation, so that with all the moulding and chisellings it has suffered, the bulk of the mass is rather increased than reduced. Nevertheless, as the preface informs us, "it has undergone alterations so extensive and fundamental that little of the original text now remains."

Inasmuch as descriptive anatomy, as such, is a limited science and of ready observation, admitting only of the same objects being described in words differing within a certain range and subject to a definite number of permutations and combinations, it follows that the vast changes referred to in this new edition must rather be looked for in other matters, and so it falls out, for the novelties and improvements are to be found chiefly in what used to be called "general anatomy," in histology, morphology, embryology, &c., in the cognate subject of surgical anatomy, and in the pointing out of homologies.

It is right also to note that "a considerable number of new figures have been introduced into the present edition, some having been substituted for former ones now withdrawn, others added as new illustrations." The engravings are very clear, but, in our eyes, the hand of the engraver has been rather heavy than artistic in many figures.

As now presented to us this treatise continues to occupy the foremost place for completeness and accuracy among British works on anatomy.

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<sup>1</sup> *Quain's Elements of Anatomy*. Eighth edition. Edited by W. SHARPEY, M.D., F.R.S., ALLEN THOMPSON, M.D., F.R.S., and E. A. SCHAFER. In two volumes, illustrated by very numerous Engravings on Wood. London, 1876.

## Original Communications.

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**Notes on Syphilis in the Insane.**—By W. JULIUS MICKLE, M.D.,  
Medical Superintendent, Grove Hall Asylum, London.

### PART I.

#### *Intracranial Syphilis and Insanity.*

IN dealing with a considerable number of men suffering from cerebral affections, or other nervous diseases, the question of the existence of syphilis as a cause or as a concomitant often arises, and is one of extreme interest, as well as of great practical importance for the direction of therapeusis. It may not, therefore, be out of place to put on record some of the cases observed by the writer, and especially so as the recent discussion on syphilis at the Pathological Society of London indicates that clinical observations, bearing upon the syphilitic nervous affections and their relations to other diseases of the nervous system, may not be without their uses. The notes which follow relate to syphilis as it has been studied in the military class of the insane patients under my care, and attention will be confined to the experience of the last three years. The class of insane private gentlemen is excluded from consideration in this investigation. The daily average number of insane soldiers under care here during the period just mentioned has been between three and four hundred, and the total number about six hundred; but I do not state any per-centage, either of the occurrence of ordinary constitutional syphilis or of syphilitic affections of the nervous system among them, as it is thought that such statistics would prove fallacious. That they *would* prove fallacious is probable for these reasons: (1.) It is often difficult to obtain a history of the patient's antecedents. (2.) Being insane, many of the patients are mentally unfitted to give the desired information, their memory is apt to be treacherous, their inattention and self-neglect extreme, and the reports of some are based upon delusions. (3.) It is particularly in instances where syphilis affects the nervous system that its evolution is sometimes insidious; that its later lesions are not preceded by its usual characteristic development on the exterior of the body, or by only a partial, or slight, or transitory development; and that the

diagnosis is surrounded by obscurity. This is the opinion of several writers<sup>1</sup> on the subject, and more than once the fact has forced itself upon my attention. The difficulty and obscurity which arise from this cause reach their height in dealing with the insane, partly for the first two of the reasons just mentioned, and partly because syphilis, though frequently the cause of nervous symptoms, is often in them the accidental associate of grave disorders of the nervous system, having an origin independent of syphilis, complicating its effects, or even overriding them. But the vital importance of the recognition of the syphilitic element when it holds place as a cause, and of tracing the effects of the virus and of the sequelæ, reaches its acme in these very cases, and one must attempt to read them by the light of observations made on the more simple examples occurring among the sane. Almost every day it is necessary to decide whether or not active or smouldering syphilis is present in the system, and, if present, whether it is the cause, or one of the causes of mental aberration and of nervous lesion; or whether it modifies these; or, finally, is a comparatively innocuous and accidental complication. In some quarters a considerable variety of clinical symptoms have been fathered upon syphilis where the affiliation might well be disputed, in others the pathological anatomy of syphilitic affections has been extended too widely. It will probably be thought that in practice I have under-estimated the importance of the syphilitic element rather than the reverse, when we recur to the fact that the instances of syphilis—cerebral or other—about to be mentioned, were derived from about six hundred insane soldiers, who have been under care here during one part or other of the period previously mentioned. These men, for the most part celibate, in the vigour of masculine life, many of them careless, reckless, and comparatively ignorant of consequences, and usually drawn from a class in whose untrained minds the passions have but little control, are notoriously liable to contract the specific lues. Many are the victims of Bacchus, and not a few suffer from the wounds of Venus, while some veterans carry in their bodies the effects of both. About forty cases will be mentioned. In a still larger number of other cases more or less evidence of syphilis existed, but for various reasons they will not be detailed here. The cases will be described in three parts, of which the present communication forms Part I. It contains cases in which organic syphilitic lesions affected the brain, meninges, or intracranial nerves.

In the second part will be found; (1), a résumé of the various

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<sup>1</sup> Broadbent, 'Lettsomian Lectures' (1), "On Syphilitic Affections of the Nervous System." Buzzard, 'Syphilitic Nervous Affections,' p. 42.

<sup>2</sup> In deciding as to the continuance of constitutional syphilis, the persistent multiple indolent bubo has not been found, in these cases, to have the diagnostic importance assigned to it by Surgeon Venning, 'Clin. Soc. Trans.,' vol. viii, p. 62.

instances of insanity in syphilitics, with reference to the varieties of so-called "syphilitic insanity; (2), instances bearing on the etiology of acute forms of insanity intercurrent in secondary syphilis; and (3), cases which bear upon the alleged connection between aortic endarteritis and syphilis.

The third part will refer to the relations existing between brain syphilis and general paralysis.

## PART I.

### *Cases of Intracranial Syphilis.*

Two quotations embody all that need be said in preface to these cases; one, that the whole group of syphilitic nervous affections "are primarily non-nervous;"<sup>1</sup> the other, that "it is not in individual symptoms, or in any arbitrary and unexplained difference in the phenomena that we shall find the distinctive characters of syphilitic affections of any part of the nervous system."<sup>2</sup>

CASE 1.—*Dementia; physical signs simulating general paralysis; headache; epileptiform attacks; transitory hemiplegia; recurrence of epileptiform convulsions, assuming an excessively violent form, and occurring in rapid succession; death in the "status." Syphilitic disease of the brain, cerebral arteries. Spleen, liver, skin, and testis.*

A private in the 11th Regiment was admitted in October, 1874, age 41, service 16 years; previously under treatment at Devonport and Netley. The cause was stated to be uncertain. The history received with him was to the effect that mental disease was insidiously developed about July, 1874, but that his conduct had been reported as being peculiar for some time previously, and that the symptoms partook of the nature of dementia from the first.

The medical certificates from Netley testified to his incoherence in conversation, confusion of thought, and failing comprehension. Further, that memory was almost lost, that he was inclined to be destructive and filthy, and was restless at night, fancying then that the men of his regiment were about him; and that he suffered from increasing general paralytic disability. Secondary dementia and paralysis of insanity are both mentioned as the form of his mental disease.

*On admission.* Physical state: Height 6 ft., weight 12 st. 2 lbs. There is some fibrillary tremor of the tongue when protruded, pupils ordinary, except that the left one is at times slightly irregular in shape, facial capillaries dilated, features full and the lines of expression partially obliterated, speech deliberate and at times faintly hesitating, but not accompanied by facial tremors. The left cheek is covered by depressed, irregular, whitish cicatrices lying in an ele-

<sup>1</sup> J. Hughlings Jackson, 'Journ. Mental Science.'

<sup>2</sup> W. H. Broadbent, 'Lettsomian Lectures' (II).



vated, dull red, vascular area of the skin, which at one point assumes rather a tubercular appearance. Similar, but smaller, patches exist on both temples and on the right cheek. He says he has no headache now, but has suffered much from frontal pain, and feels pain around both thighs, especially when he gets warm in bed. Writes legibly but with a little shakiness. Is slow and inactive in all his movements, and there is just perceptible a slight dragging of the left foot. There are some rounded, whitish superficial cicatrices on the right forearm, and a dull purplish irregularly depressed one just above the left elbow. These he attributes to "venereal." Two pale little tubercles are seen on the nape, there is a slightly indurated scar on the skin of the prepuce and traces of other sores. Inguinal and cervical glands rather well marked. Chronic indolent enlargement of the right testicle. Passes urine and fæces involuntarily during the examination.

*Mental state.*—Is highly amnesic and emotional. Is slow in comprehending and answering any question. The loss of memory is so great that he utterly confuses times and places, sometimes forgets that he has been at Netley, and speaks of the occurrences of several months ago as having happened yesterday, and says he "saw his regiment on parade last week."

He was received here without any history of syphilis, but from the general physical and mental condition, as noted above, the diagnosis was made that he was the subject of cerebral disease of syphilitic origin, and he was placed on ten grains of iodide of potassium, with six of carbonate of ammonia, three times a day. Recently I have been able to ascertain the fact that he contracted syphilis while serving in the army, but the date and place are not ascertained. On December 10th he had two severe convulsive attacks—7.30 and 10 a.m., and at 1.30 p.m. there was slight spasmodic action on the right side of the face, the pulse being 100 and the temperature 102°. Bromide of potassium was added to his mixture and nothing further of a special nature occurred at that period. After four months and a half of treatment the above mixture was temporarily discontinued on April 14th, 1875. At this date he had improved greatly in mental condition, was bright and far more intelligent than on admission, and had regained his memory to a considerable extent. He was cheerful and took pleasure in joining in the occupations and amusements of his fellow patients. He also wrote long coherent letters to his friends containing many scriptural quotations. He had grown much stouter. The tongue was still a little tremulous on protrusion; the purplish discoloration of the cicatrix above the elbow had faded away. He continued in this condition until May 12th, when he was found to be confused and stupid, the articulation was impaired and mumbling, and there was incomplete left hemiplegia, especially of the face and upper extremity. It was not

known that any convulsion had occurred. The mixture was resumed and was given every three hours, so that he took eighty grains of the iodide daily. Next day, the hemiplegia, though not quite so decided, was still very evident, and the saliva was dribbling from the left side of the mouth. He was still confused and was rather obstinate and impatient. For two or three days after this he improved, but had several slight epileptiform attacks. On the morning of May 17th, excessively severe convulsions came on in rapid succession. For several hours he had as many as twenty in each hour, lying perfectly unconscious in the epileptic status, with swollen livid purple face, and eyes deeply injected and red. The fits were quick both in their onset and in their subsidence. The course of each fit was mostly as follows. It began with tonic spasm of the right side of the face and mouth, which were drawn thereby towards the right. The head and eyes turned to the right, the right orbicularis palpebrarum partially closed in tonic spasm, and the right forearm was raised and drawn forward. Then immediately clonic spasm came on and affected in rapid succession the right side of the face, the mouth, and orbicularis palpebrarum, the eyeballs were convulsively jerked and clonic convulsion extended to the right upper, and to the right lower, extremity. The left side of the face and the left eyelids became involved also, and there was a good deal of working of the jaws. In the upper extremity there was, at the very first, flexion of the forearm which was drawn upwards, and the elbow was thrown out from the side. In the clonic spasm which followed, and was most marked at the upper arm, the elbow was held flexed at right angles and was jerked backwards, the thumb was kept straight and the fingers were flexed rather firmly on the palm. Three things occurred almost simultaneously at every onset: the mouth and face were drawn towards the right, the head and eyes turned to the right, and the arm was drawn upwards and outwards, the forearm upwards and forwards. Then almost instantly clonic spasm came on, and in the later fits was seen first on both sides of the face. When the convulsion was fully developed the face and mouth continued drawn towards the right, the right side of mouth being the more widely open, the head and eyes still turned to the right; the convulsive action was more marked in the right than in the left orbicularis palpebrarum but was always equally developed in the two sides of the occipito-frontalis. It was seen also in the left face; the muscles of both sides of the trunk were implicated in the more severe fits, and in them the left thigh jerked also. In the short intervals between the fits there was sometimes twitching about the right angle of the mouth. After seven hours the fits ceased but the pulse still rose in frequency (90 to 110), respiration was heavy, laboured, and stertorous, and the lividity and congestion of the surface marked. The perfect coma, also, continued after the fits had

ceased and the limbs were perfectly relaxed and inert. Nothing that was done had any effect in checking the fits, or relieving the coma, and he died thirteen hours after the initial violent convulsion of the series.

*Post-mortem.*—Body covered with a thick layer of fat. Calvarium thin. All the arteries at the base of the brain stood out like rather firm cords, their walls being much thickened. This thickening also involved the walls of the various branches of the main cerebral arteries, and was well seen in the vessels overlying the corpus callosum and lining the walls of the great longitudinal fissure. Besides this there were circumscribed patches of deposit in the walls of several arteries, and at these points the coats were very greatly thickened and nodulated. One was found in the walls of the right middle cerebral artery, about an inch from its commencement, diminishing the calibre of the vessel internally, and projecting externally in the form of a pair of small flattened buds, on the surface of which were minute meandering vascular twigs. In the interior of the vessel a dark clot was rather firmly adherent at this point. A similar nodulation was noticed in several of the smaller branches of this artery. In the left middle cerebral artery a somewhat similar change existed just external to the anterior perforated space. But in this vessel the adventitious material affected the inner coats more than the outer, and formed a plate of whitish fibrous-like tissue encircling the interior of the vessel for the length of about half an inch and lessening its calibre. The smooth internal lining membrane of the artery continued unbroken over the inner surface of the morbid material. The branches of this vessel also presented little nodular points and plates. Nodular or more flattened patches were found also in the upper part of the basilar artery, in both posterior cerebral arteries and in both anterior cerebral arteries, especially on the right side. Where nodulated or bulbous, the vascular walls were firm, and had either a whitish or a yellowish-white colour. The sinuses and veins of the meninges were everywhere engorged and distended by dark blood. Slight opacity of the arachnoid over the frontal and parietal lobes. The conjoined arachnoid and pia mater were rather thick and tough and slightly œdematous. These changes were especially on the fronto-parietal region. The membranes stripped readily from the surface of the cerebrum, except over the gyrus supra-marginalis and the first and second temporo-sphenoidal convolutions of the right hemisphere, where layers of the cortex, softened as will presently be described, separated along with the meninges. The cerebral matter, generally, was of slightly diminished consistence. Both the grey and white matter were

hypervascular, the grey having a mottled aspect and faintly purplish colour in front. The white substance was of a mottled lilac colour, and its puncta extremely numerous. The choroid, plexus, and the veins of Galen were congested, the fornix rather soft, and there was a moderate amount of ventricular fluid.

Over the front border and under surface of the corpus callosum, and extending to the convolutions on both sides, but especially on the right, an area of very marked gummatous disease was found. It affected part of the median plane surface and part of the orbital surface of the right frontal lobe. The anterior and inner (median) portion of the first (superior) frontal gyrus was the part mainly affected, also the front of the gyrus fornicatus, and, on the orbital surface, the gyrus rectus. The membranes covering this area were extremely thick, tough, and adherent to the cerebrum; whilst the grey cortical matter was mostly replaced by a firm yellowish exudation, which was closely adherent to the membranes on the one side and to the medullary matter on the other. In fact, the deposit was continued into the medullary matter, invading it irregularly and producing an indurated yellow layer. The induration of the medullary substance was particularly marked in front of the base line of the lateral ventricle, and in the portions adjoining the median plane surface of the frontal lobe. All these changes were less marked and occurred over a much smaller area in the corresponding portions of the left hemisphere, the areas of disease being continuous with each other over the corpus callosum. The right corpus striatum was atrophied and collapsed on part of its upper surface, the alteration commencing about a third of an inch from the inner border, and being situated mostly at the external aspect. On section, the whole thickness of the striate body was found to be diffuent in the portion corresponding to the change mentioned on its upper surface. The right and left optic thalami and left corpus striatum were deeply injected. The right temporo-sphenoidal lobe was softened and became diffuent on slight pressure, except on its inferior surface and its posterior termination. The meninges were thick and hyperæmic over the pons and medulla oblongata, the substance of which was injected, especially the posterior part of the medulla. The consistence of the medulla was rather less than usual. The lining membrane of the fourth and lateral ventricles was firm. Weight of right hemisphere  $24\frac{1}{2}$  oz., of left  $23\frac{1}{4}$  oz., of cerebellum 6 oz., and of pons and med. obl.  $1\frac{1}{8}$  oz.

The heart was covered with a considerable layer of external fat and, with this, weighed nearly 14 oz. The right chambers, *venæ cavæ*, &c., were gorged with dark fluid blood and clot; the left chambers contained a moderate amount of the same.

There was slight atheroma of the mitral valves and aortic arch. Muscular substance slightly soft and friable. Lungs congested, œdematous, and the bronchi contained much watery fluid.

*Kidneys* congested. The capsule of the left kidney was slightly adherent to the cortex and split up into layers; right  $5\frac{1}{2}$  oz., left  $6\frac{1}{2}$  oz. The spleen weighed 9 oz., and was of a purplish colour. Upon it were seen several starred cicatrices near the upper end; one other such on the anterior border where the capsule was greatly thickened, and a still more extensive one near its lower extremity. The fibrous material penetrated into the parenchyma.

The liver weighed 67 oz.; its tissue was pale and softer than usual. On the upper surface of the right lobe were five depressed white cicatrices, and the cicatricial fibrous tissue extended from some of these to the depth of one and two inches into the glandular substance, but beneath the other were found firm yellow gummata. The gall bladder contained 3 oz. of bile.

Under the microscope, sections from the yellow firm patch found adhering to the surface of the brain, when stained, showed fibres, numerous ill-formed corpuscular elements of irregular size and shape, with molecules and granules. The walls of the vessels were thickened uniformly, appearing dense, and as if infiltrated with fine granular material; two little fusiform dilatations were seen, as well as fibres, fatty molecules, and pigmented masses. The vessels of the overlying meninges were thick-walled. The subjacent brain substance stained badly. In some sections were colloid bodies, and the walls of the vessels were enormously thickened and had the appearance described above, and on the exterior of some was an opaque incrustation, of a dull red colour from absorption of the staining fluid. The right temporo-sphenoidal gyri, examined when fresh, showed fuscous degeneration of the larger nerve-cells, and the vessels were altered much as described above. In the posterior part of the first (superior) left frontal gyrus the vessels were thick-walled, some were shrunken, occluded, and surrounded by clear spaces, and there was slight degeneration of the nerve-cells. In the upper part of the left ascending parietal convolution, the vascular coats were thickened, the angles of the nerve-cells were rounded and their nuclei indistinct, while some had a moderately granular appearance. The pia mater from the upper part of the left frontal lobe showed only doubtful thickening of the coats of the vessels. The hepatic cells contained more fat than usual.

*Remarks.*—In the above case severe syphilitic disease is found

to affect the surface and the arteries of the brain. Affecting the cortical grey matter, both directly and also indirectly through the medium of vascular disease, it gives rise more particularly to mental derangement and convulsion; affecting the arteries, it increases the gravity and multiplicity of the symptoms, and undermines the nutrition of various encephalic districts, or starves them secondarily. The former lesion assumed in this case the character of a local gummatous infiltration and encephalitis, invading and indurating the medullary substance, destroying the cortical grey matter, rendering the soft membranes at that part thick and tough, and binding them firmly to the subjacent sclerosed white substance. The microscope showed that at the time of death the products were those of declining action and decay, ill-formed and caseous, and mingled with a few traces of decayed blood caught and detained along with the gummatous material, owing to the high vascularity of the part affected.

The second morbid change—the extensive thickening and gummatous nodulation of all the great arteries at the base, partially occluding some and impairing the adaptive elasticity of all—no doubt disordered the cerebral circulation, and finally led, by the way of thrombosis, to local ramollissement of the cerebral substance. But not only were the vessels at the base thus affected, their branches also partook of the same change; nor did the thickening stop at the earlier arterial subdivisions, but extended to the arterioles, and was seen in the minute microscopic vessels of most of the sections made. Looking at these no surprise could be felt at the mental deterioration which had occurred, and they suggested an auxiliary cause of the convulsive phenomena. This syphilitic arterial lesion is one of great interest and was found in the next case also. As it is convenient to speak of the arterial disease of both patients here, it may be stated, in anticipation, that in Case 2, the cerebral arteries were immensely thickened, the vessels standing out in relief like pieces of whipcord. All the arteries at the base and their branches for, at least, some distance from the circle of Willis, partook of this alteration, though whether the fibroid change extended or not to the arterioles and capillaries was only determined by microscopical examination in two regions. In addition, there was a gummous-like alteration of the wall of the left Sylvian artery. In the few sections made the minute arteries possessed unduly thick walls; in some the coats appeared to be easily separated, giving a concentric annular aspect to the section, and one or two were occluded.

The importance of the syphilitic arterial lesions of the brain is now claiming more general recognition. Thrombosis, local softening of brain, and cerebral hæmorrhage often follow in their train, and when the morbid change is extensive the termination is probably always fatal. The vessels at the base may be affected indirectly; a syphiloma or a gummatous infiltration pressing upon them, occluding them partially or completely, and leading to thrombosis, to softening,<sup>1</sup> &c. But in the lesions now specially under consideration the arteries are affected directly and primarily by syphilitic disease, and not by the mere external pressure of syphilomata. In some there are nodular masses, gummatous patches, embedded hard grains, or circumscribed local thickenings in the walls of the larger cerebral arteries. Of this kind are cases detailed by Wilks,<sup>2</sup> Hughlings Jackson (his cases four in number),<sup>3</sup> Lancereaux (two),<sup>4</sup> Moxon,<sup>5</sup> Broadbent,<sup>6</sup> and Huebner (two).<sup>7</sup> Frequently clots are adherent to the internal walls of the vessels at these points, usually softish, but sometimes so firm and large as to form tough, adherent, cylinders obliterating the channel, as seems to have occurred in Bristowe's<sup>8</sup> case.

In others, there is found a *general* extreme thickening of the walls of all the arteries at the base of the brain from infiltration and deposition of adventitious material, and this, in several cases at least, has been found to extend more or less throughout the subdivisions of the arterial trees to their most minute ramifications, and not only in the cerebral substance but in the meninges also. Examples of this are described by Clifford Allbutt,<sup>9</sup> and a less marked case by Huebner.<sup>10</sup> The surface of the basal vessels is more or less irregular in these cases.

Not unfrequently the marked nodulation of these more or less universally thickened basal vessels, or the presence of gummatous patches in their walls, gives rise to a combination of the two conditions above described. Of this nature are the instances given by Batty Tuke,<sup>11</sup> Buzzard,<sup>12</sup> and my own two cases (Cases 1 and 2).

To whichever variety the vessels pertain, their condition is one

<sup>1</sup> Lancereaux, *Treatise on Syphilis*, Syd. Soc., vol. i, p. 401, who also quotes instances from Gräfe, Virchow, Böning and Passavant. Moxon, '*Med. Times and Gaz.*,' vol. i, 1871, p. 712. Broadbent, *op. cit.*

<sup>2</sup> '*Guy's Hosp. Rep.*,' vol. ix, p. 45.

<sup>3</sup> '*Journ. of Mental Science*,' 1874, pp. 235, 239; '*London Hosp. Rep.*,' vol. iv, p. 314.

<sup>4</sup> *Treatise on Syphilis*, vol. i, loc. cit.

<sup>5</sup> '*Guy's Hosp. Rep.*,' vol. xiii, p. 332.

<sup>6</sup> *Op. cit.*, lect. iii.

<sup>7</sup> Quoted by Buzzard, p. 19.

<sup>8</sup> '*Path. Trans.*,' vol. x, p. 21.

<sup>9</sup> '*St. George's Hosp. Rep.*,' vol. iii, and '*Path. Trans.*,' vol. xiii, p. 8.

<sup>10</sup> Quoted '*Journ. Ment. Sci.*,' 1874.

<sup>11</sup> '*Journ. Mental Science*,' Jan., July, and Oct., 1874.

<sup>12</sup> '*Syphilitic Nervous Affections*,' p. 64.

totally distinct from atheroma, on the one hand, and, on the other, from the hypertrophy found where the circulation of imperfectly depurated blood has produced habitual overaction, and consequent overgrowth of the arteriolar tubes. Neither the naked eye nor the microscopical appearances are those of the former—atheroma,—and as for the latter change, it is different in its distribution and is associated with disease of the principal emunctory organs. It will be noticed that the arterial lesions partake of the diversity of appearances observed in every organ and tissue which is liable to syphilitic changes. Here, as elsewhere, the lesions may be diffused or circumscribed, and their concomitance with syphilitic deposit in other organs, or with other evidence of syphilis in or about the brain, shows that they are originated by the lues venerea. Besides my own, several of the cases quoted above occurred in soldiers. May this be explained by saying that injury or overstrain of any particular part in a syphilitic person inclines to the development of syphilitic lesion in that part, and that the vascular system of soldiers, owing to their drill, accoutrements, &c., is peculiarly liable to overstrain? This, however, would scarcely explain the localisation of the disease in the cerebral arteries.

In the above case it was seen that the minute vessels of the meninges and of the cerebral convolutions had their walls considerably increased in thickness, though not in an entirely uniform manner. All the coats of the minute arteries seemed to be implicated. The nodules in the coats of some of the large arteries at the base bulged from the exterior like little flattened buds. But at the same point was an annular fibrous-like deposit, lying in the inner coats and partially occluding the lumen, and covered by, at least, the bright smooth endarterium. On making sections the deposit was slightly separable from the coats by teasing. But only so at the edge, where it was thin and in process of being bevelled off,—the main portion of the infiltration was intimately bound up with coats of the vessel, and a similar disposition of parts was observed in the Sylvian artery of Case 2.

Dr. Batty Tuke describes a case in which extensive disease of cerebral vessels, supposed to be syphilitic in nature, had led to softening, apoplexy, &c. The basal arteries were much thickened, and some of them nodulated, and made moniliform by large yellowish deposits. Microscopical examination showed the muscular coats of the vessels of the pia mater to be much thickened, and there were irregular swellings produced by molecular matter. In some sections from various parts of the brain and cord there was thickening of the muscular, in others, of both the muscular and the outer fibrous, coat of the minute arteries. "Surrounding the latter coat were concentric rings of a material in which were held corpora amylacea; in some instances empty spaces existed between this material and the brain substance,



in others, this interspace was filled with a colloid-looking substance. In many of the smaller arteries perfect occlusion had taken place."<sup>1</sup> The nearer to the diseased cerebral tracts the greater were the vascular changes. The minute arteries of the cerebellar pia mater, of the pons, and of the cord were similarly affected, and there were immense deposits of hæmatoidin beneath the ependyma ventriculorum and in the perivascular canals. It must, however, be noted with reference to this case, that no syphilitic deposit was found in any other organ, that much of the change seems to have been perivascular rather than of the vessels, and that the left ventricle of the heart was hypertrophied. Dr. C. Allbutt<sup>2</sup> mentions another case; that of a soldier. There was extensive disease of the basal arteries; they were thickened, firm, dense, or nodulated to the touch, and had a whitish-yellow tint. The smaller vessels were also much thickened and altered in every part of the encephalon; every artery and arteriole seemed to be affected. In the external vessels the change was "chronic arteritis with great nuclear and cellular proliferation and affecting all the coats to some extent, but especially the inner and middle coats." In parts the distinction between the coats was lost, or the middle coat was separated from the inner by nests of nuclei and granules, or the surface of the inner coat was gone at points, through some of which little pear-shaped processes projected. The walls of the thickened arterioles were the site of active nuclear proliferation, and their perivascular canals were distended by a transparent fibrillar material. In sections of some hard little masses in the encephalon the thickening seemed to be more around than in the vascular walls, the minute arteries and arterioles lying "like tortuous cords embedded in green transparent granular matter." The liver and thyroid glands were considered to exhibit syphilitic changes. In a case recently published by Dr. J. J. Brown<sup>3</sup> the muscular coat of the small arteries of the encephalon was hypertrophied, the outer fibrous coat thickened and surrounded by a molecular deposit containing granular masses. The morbid change, however, was mainly perivascular, and proof was wanting that it was of syphilitic origin. There were no other post-mortem indications of syphilis, and the evidence afforded by the mental symptoms was ambiguous. Lancereaux describes a syphilitic neoplasm in the walls of the internal carotid arteries, at their termination, as formed of rounded nuclei and some cells of connective tissue. It appears<sup>4</sup> that some very recent observations by Huebner and Ewald locate the new syphilitic formation in the inner coat of the smaller arteries beneath the epithelial lining, and they

<sup>1</sup> 'Journ. Mental Science,' 1874.

<sup>2</sup> 'St. George's Hosp. Rep.,' vol. iii, and 'Path. Trans.,' vol. xxiii.

<sup>3</sup> 'Journ. Mental Science,' 1875, p. 270.

<sup>4</sup> 'Brit. Med. Journ.,' Jan. 29, 1876.

describe it as a tumour of the connective-tissue type, which may finally be differentiated into a structure resembling the normal arterial coats, or may assume the form of simple connective tissue.

Since writing the above a note has appeared<sup>1</sup> on an article by Friedländer of Strasburg, stating that he denies the condition described by Huebner to be specifically connected with, or depended upon, syphilis, and asserts that it is a form of the "obliterative endarteritis" described by himself.

When wide-spread the condition seems to be a chronic arteritis of syphilitic origin, the acute form of which has been described by Dr. Moxon.<sup>2</sup> When circumscribed it often appears to partake of the nature of ordinary syphiloma.

The symptoms which arise from extensive syphilitic disease of the arteries of the brain are of great variety, and are usually complicated by the symptoms arising from the presence of other intracranial syphilitic lesions.<sup>3</sup> Taking a number of cases, however, just as they occur, and with all their complexity, the order of relative frequency of the symptoms is as follows: (1), convulsive symptoms of various kinds; (2 and 3), dementia and hemiplegia; (4), apoplectic symptoms, or coma, drowsiness, somnolence, especially in the later periods, while a great variety of mental symptoms occur in the earlier periods. Headache, aphasia, difficulty of articulation, palsies of cranial nerves, are often found; and vomiting, blindness, and optic neuritis are not infrequent. Many of the cases formerly described as encephalitis of syphilitic origin belong, probably, to this series. But the symptoms of the diffused encephalopathies (encephalitis), as described by Lancereaux,<sup>4</sup> differ from those of the arterial cases collected above, in the less frequency of convulsive attacks and of lesions of the intelligence, and in the greater frequency of rigidity or contraction. In Case 1 the arterial disease and its secondary effects were complicated by the gummatous disease of the cerebral surface and of the soft meninges. In a group of cases showing such lesions (excluding the affection of nerves at the base) the order of frequency is headache and convulsive seizures of the nature of hemispasm; tremor, partial, incomplete, or transitory hemiplegia; contraction, or anæsthesia of a limb; mental symptom; optic neuritis, &c. The succession of symptoms in Case 1 thus complicated was:—dementia, the earlier course of which was chequered by restlessness, destructiveness, and irritability; early headache; epileptiform attacks; transient slight hemiplegia; impairment of motor energy; recurring epileptiform

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<sup>1</sup> 'British Med. Journ.,' March 18, 1876.

<sup>2</sup> 'Lancet,' vol. ii, 1869, p. 435.

<sup>3</sup> For thromboses of syphilitic arteries, see Broadbent in the 'Lancet.'

<sup>4</sup> Op. cit., vol. ii, p. 59.

seizures, with mental confusion; and a lethal attack of furious convulsions.

The principal lesions of the surface of the brain (and the softening of its substance) were on the *same* side as that on which the convulsions which proved fatal began, and in which they continued to be most developed when they became bilateral. But if the fits, on the other hand, be attributed to the only gross lesion of the *left* hemisphere, a comparison of the site of the lesion with the course and distribution of the convulsions shows that the case affords no support to recent views, based upon experiment, as to the cerebral localisation of movements.

The diagnosis will be more specially adverted to in Part III.

*CASE 2.—Sudden attack of paresis of articulation and deglutition and of incomplete right hemiplegia, followed by intellectual and moral deterioration, and by temporary attacks of dysphagia with vaso-motor disturbance. Subsequently, dementia; incomplete, right hemiplegia; speechlessness; intestinal torpor; contraction of limbs. Extensive disease of cerebral arteries and gumma of left corpus striatum.*

An artilleryman, aged 33, was suddenly seized with difficulty of articulation and deglutition, and some dextral hemiplegia. The history is that mental derangement then came on, and the patient was confused, excitable, angry, quarrelsome, and at times impulsively violent. He stole whatever he could lay his hands on, and his habits were indecent and dirty. Several attacks of temporary disability to swallow also occurred, and were preceded, in each instance, by great flushing of the face. He had suffered from syphilis and ague whilst serving in India, but the dates of these were not known.

He was admitted into the asylum six months after the onset of the first symptoms. There was considerable mental failure, loss of memory. Though he comprehended very simple questions and could write a few words with extreme slowness and hesitation, he virtually was only able to reply by signs, as speech was nearly abolished and he could only utter a few mumbling unintelligible sounds. He was short and stout, the features were bloated, flabby, and apt to become purplish, the look was fatuous, the tongue tremulous, and there was incomplete dextral hemiplegia, affecting the face and both right limbs. Some doubtful recent cicatrices and scaly patches were seen on the back. He improved both physically and mentally for a time and then steadily retrograded. Throughout there was more or less dementia, but without any of the intercurrent impulsive outbursts to which he had been subject before his admission, and the habits were no longer degraded. Latterly, there were difficulty of swallowing, obstinate constipation, drowsiness by day and sleeplessness at night; frequently, moaning or groaning, constant and

increasing contraction of the limbs, especially of the right limbs, which were cold and purplish ; and a greater degree of hemiplegia. He died nearly eleven months after admission. The treatment was mainly with iodide of potassium.

*Autopsy.*—Calvarium thick and dense, Pacchionian depressions large and shallow. The arachnoid was thickened generally and presented some milky opacity over the superior and lateral surfaces of the cerebrum. Over the middle of the vertex of the brain there was a little serum lying in the sulci, and the pia mater was thickened and hypervascular over the superior aspect of both hemispheres. The soft membranes stripped off with ease. Ventricles rather large, fornix soft, dark clot in the veins covering the walls of the left lateral ventricle. Grey matter of cortex pale, white matter of ordinary appearance.

All the arteries at the base of the brain were extremely thickened and stood out like firm cords. They were slightly irregular on the exterior, but the thickening was very uniform and seemed to arise from new material, the appearance of which had become assimilated to that of the normal coats. There was a brownish-yellow swelling, about one third of an inch in length, on the left middle cerebral artery, immediately external to the substantia perforata. The walls were greatly thickened at this point, the calibre of the vessel lessened, and its interior filled with a dark soft clot, and fine dissection showed that the coats were here readily separable from each other. The change has already been more minutely described in the remarks following Case I. The microscope showed the walls of the minute arteries in the third left (inferior) frontal convolution to be considerably thickened, and some of the smaller ones were found occluded. The same increased thickness of the walls of the arteries was found in and about the left corpus striatum, and the new material seemed to be disposed in concentric layers. The intra-ventricular aspect of the left corpus striatum was sunken and discoloured over a large area, and the greater part of the ganglion was replaced by a firm, tough, yellowish, gummatous mass, which, however, did not invade any of the neighbouring parts. Cerebrum 42 oz., cerebellum 5 oz., pons and med. obl.  $\frac{1}{4}$  oz.

The *heart* was softish, flabby, and weighed 9 oz ; its valves were healthy. *Lungs* congested and cedematous, on the posterior surfaces. At the base of the right lung, close to the diaphragm and towards the anterior border, were two small vomices, around each of which there was a little zone of pulmonary consolidation. No tubercle seen anywhere. On the upper surface of the *liver* was a large, brownish-yellow, gummatous mass. The kidneys were healthy.

*Remarks.*—As a matter of convenience the arterial lesions of this case have already been discussed under Case I. The case is incomplete in some aspects, there having been no microscopic examination

of the medulla oblongata and pons. Hemiplegia dependent upon syphilitic disease is rarely caused by the particular lesion found in this case; that is to say, by a gummatous mass situated in the motor tract.

*CASE 3.—Dementia; cachexia; excessively feeble heart's action, circulation, and reparative power at the periphery; cardiac dyspnoea; death from cardiac failure. Syphilitic disease of heart, cranium, cerebral meninges and cortex.*

A private of the 16th Lancers was admitted in May, 1873, at the age of 34. Mental disease was stated to have existed for about ten months, and was thought by those who sent him here to have been aggravated by the influence of a tropical climate, but he seems to have returned from India in 1871, some time previous to suffering mental deterioration. No history of syphilis was received with him, but I have lately obtained the following regimental records:—"Primary syphilis" in 1862, again in 1864, and again in 1868, the first two entries at home stations, the last in India. Then "anæmia" in 1869 (India); "hepatitis" in 1871; "muscular rheumatism" in 1872 (home). It appears that at Netley the symptoms were great loss of memory, confusion of thought, incapacity to form a judgment on, or to act in, any matter, occasional irritability and mischievousness, and a circulation so feeble that the extremities were with difficulty kept alive.

*On admission here.* Height 5 ft. 7 in., body well covered by external fat. All the tissues were soft and flabby, and the face wore an unhealthy pallor. The margins of the ears, the tip of the nose, and part of the skin on the dorsum of a foot had disappeared, and the tissues at these points were livid, purplish, and covered by slight unhealthy crusts following indolent ulceration. It was said that this arose from little frostbites, which affected him on the voyage home, and destroyed the skin at these parts. So low was the reparative energy that no durable healing had ever taken place. The respiratory murmur was very feeble, percussion sounds very clear. The heart's action was rapid and excessively feeble, the sounds were heard very faintly, and no impulse could be felt. The lung seemed to encroach upon the precordial area. Dyspnoea arose on the slightest exertion. There was a large irregular cicatrix in the left groin. No albumen in the urine. General weakness of the mind and very great loss of memory existed. He fancied his friends were near him, but this seemed to arise from utter confusion of times and places. It was necessary to keep him in bed most of the time, for whenever he sat up the old ulceration sites turned purplish and cold, and the imperfect cicatricial tissue or the crusts covering them began to break down. He complained now and then of the pain in these parts, but not much of headache. During the last few

weeks of life he suffered from occasional diarrhœa, which enfeebled him still more whenever it came on, and from attacks of vomiting. On the night of May 11th, 1874, gastric symptoms again set in; there were marked dyspnœa, a deep purplish state of the old sores, and embarrassed action of the heart. These symptoms gradually increased and he died within twenty-four hours.

*Autopsy.*—Body fat, rigor mortis, a worm-eaten appearance of the inner surface of os frontis and centres of parietal bones. The soft meninges on the superior and lateral aspects of the cerebrum were thick, vascular, and œdematous, and separated readily from the brain. A well defined lesion was found in the right temporo-sphenoidal lobe. The grey matter was destroyed over the anterior halves of the first and second temporo-sphenoidal gyri; the meninges here were thickened and lined internally with an irregular, firm, yellowish, material, which also invaded the subjacent white substance, a thin layer of which was indurated. A patch of similar change, of the area of a half-crown piece, was found at the posterior termination of the superior temporo-sphenoidal fissure. A portion of the anterior part of the left optic thalamus, of about the size of a pea, was brownish yellow, softened and diffuent. The consistence of both the grey and white matter of the cerebrum generally was somewhat lessened, and the grey cortex was pale. Cerebrum 37½ oz. Cerebellum 6 oz. Pons and medulla oblongata 1 oz.

The heart was large, somewhat dilated and hypertrophied, and, together with the external fat which was very considerable, weighed nearly 18 oz.; all the chambers contained pale clots and a few dark clots, and the valves were healthy. The muscular substance of the heart had everywhere entirely lost all traces of the ordinary fleshy colour. It was of a dull yellow and dingy white colour, as if the muscular fibres had in some way undergone a very decided change. But in the walls of the left ventricle large portions were found of a firm fibrous texture, quite white, irregular in shape, and contrasting strongly with the surrounding dull yellow muscular tissue. To the front and left of the upper part of the inner surface of the left ventricle was a slightly distended pouch, formed by the thinning and almost complete destruction of the muscular tissue, the cardiac wall at this point consisting only of the pericardial layer and of a very thin stratum of whitish degenerated ventricular muscle. An incipient cardiac aneurism resulted from this erosion which had destroyed the endocardium and part of the ventricular wall at this point, producing an irregular excavation which was lined by a thin layer of whitish separable pale clot. No aortic atheroma. *Lungs*, emphysematous in front, slightly congested behind. *Kidneys*; capsules slightly adherent, and surfaces slightly irregular; a dull faintly yellowish colour of the cortices. R. 9 oz, L. 7 oz. *Liver*, 57 oz., slightly "nutmeggy."

*Remarks.*—No history of syphilis was procured with the patient at the time of his admission, nor were the ordinary indications of constitutional syphilis present. That he had had suppurating bubo proved nothing. Treatment was therefore directed to the general cachexia and asthenia. Long after his death the records as to his syphilis were obtained.

The lesion of the cerebral surface in this patient seems to be a later stage of a form described by Zambaco<sup>1</sup> in some of his cases.

The cardiac change was more profound than that observed in any of the syphilitic heart cases to which I have been able to refer. In Dr. Wilks's<sup>2</sup> patient a dense, more or less tough, dry, fibrous mass about the size of a billiard ball was embedded in the septum of the ventricles, and at its circumference was bleuded with the muscular tissue around. In Ricord's<sup>3</sup> case several gummatous masses were found in the walls of the ventricles, especially the right; they were yellowish, dry, firm, in parts of a schirrous consistence, in others much softer. Lebert<sup>4</sup> describes three rounded gummatous tumours in the wall of the right ventricle, projecting under the endocardium, and having a pale yellow hue ("une teinte jaune pâle"). In this, and in Ricord's, case other decided syphilitic lesions were found. Three irregular nodules also affected the wall of the left ventricle in a case recorded by Dr. John Morgan.<sup>5</sup> They were firm to the touch, and of a pale flesh or cream colour. In Virchow's<sup>6</sup> case masses of deposit were found in the heart substance, chiefly in the septum. In Dr. Haldane's<sup>7</sup> case the morbid change presented a greater resemblance to that of Case III than did that of any of those just mentioned. A flattened mass, of moderately firm consistence and of a pinkish grey colour, was embedded in the anterior surface of the left ventricle. The greater part of the septum had a greyish or yellowish pink colour, as if portions of the muscular substance had been converted into fat, and the new matter seemed to have been deposited in separate masses with slight intervals between them. "When the septum was cut through the deposit was found to extend through its entire thickness; it had a considerable resemblance to fat but was of a pinker colour and tougher consistence; a small quantity of a watery juice exuded from it on pressure; intermixed with it, chiefly near the endocardium, was a material, which, to the naked eye, resembled

<sup>1</sup> 'Des Affections Nerveuses Syphilitiques,' p. 66.

<sup>2</sup> *Op. cit.*, p. 43, and 'Path. Trans.,' vol. viii.

<sup>3</sup> 'Clinique Iconographique de l'Hôpital des Vénériens,' Plaque xxix. "Dégénérescence plastique tuberculiforme du tissu musculaire du Cœur. Accident tertiaire."

<sup>4</sup> 'Traité d'Anatom. Pathologique,' t. i, p. 470.

<sup>5</sup> 'Dublin Quar. Journ. Med. Sci.,' vol. lii.

<sup>6</sup> Quoted by Haldane.

<sup>7</sup> 'Edin. Med. Journ.,' vol. viii, p. 440.

fibrous tissue, being tough and of a bluish or greyish colour. The wall of a portion of the left ventricle extending from the base half-way to the apex and situated near the septum was in a precisely similar condition; some of the pectinate muscles appeared to be completely converted into this material." No other syphilitic changes were found. Aitkin mentions two cases of gummata of the heart in soldiers.

In case III, the condition may be described as the result of chronic syphilitic myocarditis, and of changes in its products. An instance of the more acute form of a similar pathological process is cited by Mr. Hutchinson,<sup>1</sup> in which the muscular substance of the heart was extensively inflamed. "The patches involved were large and included, indeed, the greater part of both ventricles, especially of the left. These patches were tolerably well defined and much paler in colour than the rest, being of a yellowish grey colour." Aitkin describes the later fibrous and fatty changes in interstitial myocarditis. "White fibrous tissue abounds in the left ventricular layers of muscle and the heart is usually larger . . . . vascularity increases, yellow deposits follow, the result of fatty degeneration of the new growth; and the lesion ultimately passes to the muscular fibres of the heart."<sup>2</sup> Syphilitic disease of the heart appears to partake of the variety of forms assumed by syphilitic disease of the muscles generally.<sup>3</sup>

The clinical features of these cases vary considerably and are often very slight.<sup>4</sup> Death sometimes takes place more or less suddenly, without the patient having been under care specially for cardiac symptoms. Of this kind were the above mentioned cases by Ricord, Wilks, Virchow, Haldane, and Hutchinson. It is especially in the description of syphilitic cardiac lesions by Morgan<sup>5</sup> that I find a close similarity to the clinical aspects of my own case. In one of his cases the circulation was extremely feeble, the tip of the nose was cold and discolored, and gangrene appeared to be imminent in it. In another, the face and neck were slightly puffed and of a dingy hue, the lips bluish, the pulse remarkably feeble, the area of precordial dulness lessened, the heart's impulse scarcely perceptible to the eye and touch, the first sound diminished in intensity. The patient suffered much from coldness and lassitude, and, later on, from palpitation, precordial uneasiness, increasing debility, and at times hurried breathing. The semi-congested appearance of the face increased notably, irritability of the stomach supervened, and the patient died out rather gradually. The necropsy

<sup>1</sup> 'London Hosp. Rep.,' vol. iii, p. 382.

<sup>2</sup> 'Practice of Med.,' vol. i, p. 887.

<sup>3</sup> Wilks and Moxon's *Path. Anat.*, p. 123.

<sup>4</sup> Bäumler, 'Ziemssen's Cyc.,' vol. iii, p. 215 (Trans.).

<sup>5</sup> 'Dublin Quar. Journ. of Med. Sci.,' vol. lii, p. 42.



showed syphilitic masses in the muscle of the heart, as already quoted. In its clinical features this case reads almost like a description of the one I have given (Case III). The heart, however, was small, while in mine it was large and dilated, but nearly covered by the emphysematous pulmonary borders. Dr. Morgan<sup>1</sup> summarises the general history of these cases as follows. "(1), A primary infection probably ten or fifteen years previously. (2), Evidence of constitutional infection, rash, alopecia, or mucous patches; (3), pain, and some gummatous tumours or ulcers, increasing weakness and languor, skin below normal temperature, appetite bad, complexion unhealthy, pulse feeble and not increased in frequency, . . . . feeble systolic sound, occasionally a slight anæmic bruit and diminished impulse; after a time the febleness of the circulation increases, the pulse becomes weaker, chilliness is much complained of, and in one instance so great was the languor of the circulation that there was the greatest danger of gangrene of the face occurring."

In many cases of cardiac syphilis the diagnosis is impossible; in all, it is surrounded by much difficulty and obscurity.<sup>2</sup>

*CASE 4.—Intense cranial pains; dementia, then bien être, many symptoms of the case simulating general paralysis of the insane; symptomatic paralysis agitans; disturbed innervation of heart; impaired power of deglutition; extreme general motor paresis; inability to walk; incomplete left hemiplegia; affection of speech. Mental and physical recovery under specific treatment.*

Private in 86th Regiment, age 28, service 6 years. First attack. Attributed to syphilis. Admitted February, 1874. The history was that the patient had been many times in his regimental hospital for the effects of constitutional syphilis, whilst serving at the Cape. He was invalided on this account, and during the time he was under treatment at Netley mental derangement came on, or grew more marked, and he was transferred from the surgical to the lunatic wards of the Royal Victoria Hospital in August, 1873. Whilst in the latter wards he is said to have shown delusions, to have been restless, excitable, and the subject of hallucinations of hearing. Nocturnal syphilitic pain continued, he became irrational and childish in demeanour and language, and showed great impairment of general mental power, incoherency, and apparent incipient general paralysis. These particulars are recorded in the medical certificates under which he was sent here.

*On admission, Feb. 28.* The pulse was small, rapid and feeble; the face pale, the hands cold and livid, the skin moist, the pupils sluggish and slightly irregular in shape, the left face partook slightly of the moderate degree of left hemiplegia which was present;

<sup>1</sup> Op. cit., p. 43.

<sup>2</sup> Bäumler, op. cit.

tongue rather large, pale, protruded fairly and slightly to the left ; speech impaired, the words being clipped, indistinct and somewhat mumbling. On one occasion facial and labial tremor was noted during speech ; subsequently, this symptom was never present in any decided form. There was difficulty in swallowing, and he was fed with fluids and sops. The head and neck were affected with spasmodic tremors (local paralysis agitans). He said that he saw better with the left eye, and that a few weeks ago he now and then "saw double." There was some paralysis of the left face and left limbs, but all his movements were devoid of vigour. He could not walk many yards without falling ; the gait was uncertain, swerving, and the feet were shuffled along the floor. The grasp of his hands was feeble, especially that of the left ; it was only with great difficulty and awkwardness that he could succeed in buttoning a button on his coat, and it was with extreme slowness that he managed to sign his name, the writing being, also, shaky and erratic. He had a fatuous heavy look, but through it all the aspect was often smiling and pleased. At that time the mental symptoms were simply those of self satisfaction, of *bien être*, and of intellectual and emotional failure and weakness, the loss of memory being very marked. He was, however, questioned as to the venereal disease. He said he had had hard chancres eight years before, and others since. There were no external indications of syphilis, past or present, except a minute cicatrix on the soft palate. On the glans was a faint cicatrix of some sore, in both groins were the scars following suppurating buboes.

He was kept under observation, and well dieted but without specific treatment, for five days. During this period the breath was foul, the bowels confined, the pulse rapid and feeble, and there was headache and disposition to syncope. A tendency to bedsores, and a few bronchial râles, also appeared, and the general condition was retrograding. He was then ordered to take eight grains of Iodide of Potassium three times a day, and to this Carbonate of Ammonia and Tr. Cinchon. were added. Three weeks afterwards (March 26), there was great mental and physical improvement, headache was gone, the memory was better, and he had regained much motor power, being able to walk, and even to *run* a few steps. The gait, however, was still deviating and uncertain, though of course less so than before ; and a swaying and awkwardness was seen when he turned round in walking. The spasmodic tremor of the head and neck were less marked, speech was less mumbling and indistinct, the features were still greatly wanting in expression, there was no tremor of facial muscles during speech, the pupils were sluggish and the left one a little the smaller, and the pulse was much as before. So decided an arrest of the downward course of the disease, and so marked an alleviation of the symptoms, encouraged

one to proceed with the treatment under which they had occurred. On April 3rd, the dose of Iodide was increased to fifteen grains three times a day, and was given with five grains of Ammon. Carb., and on the 7th the dose of Iodide was still further raised to twenty grains three times daily. After this he continued to improve in every respect, and in August his memory for leading occurrences seemed to be good, and he talked coherently. There was still a want of emotional control, and he would laugh unrestrictedly on the slightest provocation. He was always smiling, good humoured, and often laughing without any apparent cause. Owing to deficiency of education he was almost unable either to read or to write, but he was now able to sign his name in a much more steady hand than when admitted. The hemiplegia had quite cleared away, there was none of the local paralysis agitans, no difficulty of swallowing, and no impairment of speech. Not only was he able to walk well, he was fond of the exercise, and became an arduous pedestrian, walking for hours together if not prevented. There was no facial tremor during speech, but the features were insufficiently mobile and the lines of expression were still somewhat blurred. The natural cast of the features, however, was broad and heavy. The pupils still remained sluggish and slightly irregular. He now stated that he had had venereal sores eight, three, and two years before; that the suppurating buboes occurred after the last sores, coming on, one four days, the other a week, afterwards; that the cranial pains first attacked him from two to three years previously, that they had been continual, had been worse at night, and that he felt a little of them when he first came here. He had grown stouter, but the pupils and pulse were much as before.

On one or two occasions after this, as well as before it, the improvement suffered a temporary arrest, the patient being noticed for a day or two to be rather dull, and stupid, -and, once at least, to suffer a little cranial pain. On these rare occasions he was ordered the iodide in doses of twenty grains every four hours, or a hundred and twenty grains in the twenty-four hours, and infriktion of Ung. Hydrarg. These certainly seemed to control the symptoms and, except that its course was thus slightly chequered, the marvellous improvement was maintained, and advanced towards recovery. The pulse, though not so frequent as it had been, continued above the normal average, the facial lines were deficient in clearness, and a slight tendency to laugh without cause, and disposition to be too readily thrown into great merriment by trifles still remained. His memory was good, he was perfectly rational and coherent and was discharged, recovered, in October, 1874, being enjoined to continue the iodide.

*Résumé of Treatment.*

March 5 and 16.—Potassii Iodid. grs. viii, three times a day; increase on April 3rd to grs. xv, t. d.; on April 7th to grs. xx, three times a day, occasionally grs. xx every four hours, and Ung. Hg. infric. „ „ Ammon. Carb. grs. x, reduced to grs. v, three times a day.

*Remarks.*—Interesting in itself, this case was made doubly interesting by its close simulation of a variety of general paralysis of the insane.<sup>1</sup> Indeed, it had been set down as the latter in another quarter. Had the patient been received without any history the absence of any decided external proof of syphilis, and his own incapacity to give, at that time, any very reliable evidence, would have left the clinical distinctions imperfect and indecisive. It is not the “typical” case of general paralysis, as found in the text books, that is apt to be confounded with brain syphilis having a certain order of symptoms; it is the case in which grandiose delirium is absent, in which dementia is early and holds a leading place, in which paresis is early, decided, and associated with other morbid motor symptoms. Cases of brain syphilis, simulating this affection more or less, and presenting symptoms of the kind described in this case (IV) usually depend upon a grave alteration of the nervous centres, an alteration which often leads to death at an early period, which if relieved tends to recur, and at the best is only seldom entirely and *permanently* recovered from. It is one of those forms of syphilitic disease in which the dictum of Viduus Vidius as to the pertinacity of syphilis particularly holds true, “*Magis inducias facit quam pacem.*” The disease would be liable to recur in the above patient unless he was kept persistently under treatment. The variety in the group of clinical phenomena was of diagnostic value—mental derangement and failure, universal paresis, paralysis, disorder of sensation, spasmodic tremor, dysphagia, ocular and articulatory troubles, and paresis of the inhibitory nerve supply to the heart.

*CASE 5.*—*More or less intense cranial pain for a prolonged period; convulsive seizures, and transitory paralysis. Subsequently, dementia; hemiplegia of some months' duration; amaurosis from extreme double optic neuritis; incomplete and partial anæsthesia of one side of body; much unilateral impairment of hearing; severe vomiting; recurring epileptiform attacks; intestinal torpor, and paroxysmal epigastric pains. Recovery under antisymphilitics from all these symptoms, except the blindness and occasional epileptiform seizures of a milder character than formerly.*

A private of the 101st Regiment was admitted here in June, 1875. His height was about 6 ft., weight 10 st. 13 lbs., age 25,

<sup>1</sup> See also a case in Dr. Jeffrey Marston's able report, ‘*Med.-Chir. Trans.*,’ vol. xlv, p. 94.

service more than seven years, habits temperate. Insidious in its commencement, the mental derangement was stated to have become marked since October, 1874, and its cause had been considered to be uncertain. Prior to his admission here he had been under treatment at Parkhurst and Netley and, as the diagnosis was surrounded by some difficulty, it will be as well to transcribe the principal points in the information sent to us with the patient from the latter place on June, 28th. "No history of hereditary predisposition, syphilis, or injury to head. A very meagre statement of the case was furnished" (*i.e.* to the medical officers at Netley) "in which it is recorded that he had epileptiform convulsions and paralysis of a transient nature. On admission here (Netley) from Parkhurst, he was found to have incomplete left hemiplegia, with partial deafness, total blindness, and a mental condition of progressive fatuity. He had an epileptic seizure on the 15th of June. The physical condition has improved since admission, but the mental state continues much the same. The use of very active aperients has been required from time to time," to meet obstinate constipation. Loss of memory, incapacity for prolonged conversation, confusion of thought, and, at times, incoherence, were also specified in the certificates relating to him. The abstract of his medical history sheet embraces the following entries. In 1867, dysentery—in 1869, gonorrhœa, acute rheumatism—in 1870, remittent fever—1871, gonorrhœa—1872, chronic rheumatism, gonorrhœa—1873, carbuncle—1874, febricula—1875, "softening of brain."

*On admission* to Grove Hall Asylum there was left hemiplegia, more marked in the arm than elsewhere; some little motion could be effected at the shoulder and elbow, but the fingers could not be moved in the slightest; the left arm and hand were cold and bluish, and the whole left upper extremity felt cooler than the right. Motor power was much impaired in the left lower limb, which dragged stiffly, and he could only take a few paces without falling; also in the left side of the face, especially the lower parts about the mouth, and the tongue was protruded towards the left. Incomplete tactile anæsthesia of the left side of face and of left limbs coexisted with the motor failure. The head and eyes turned somewhat to the right; by a voluntary effort he could turn them to the left, but when at ease they always reverted to the former position. He was utterly blind, incapable of distinguishing day from night by vision; the pupils were dilated and immobile, the eyeballs rolling, and the ophthalmoscope revealed a late stage of extreme double optic neuritis. There were occasionally subjective sensations, as of a flash of light, or as if a dark object shot athwart his eyes. Later on, he stated that the sight was gradually disappearing during four months. Hearing was impaired in the right ear. The right eyeball was tender on external pressure, and there was great pain and

tenderness and some swelling over the whole region of the vertical portion of the frontal bone on the right side. The pain was darting; the application of cold, he said, had always given him a little ease, but heat was disagreeable to the head. About this period his account usually was that he suffered from severe cranial pains every day for about a year and a half, and that it was always situated either across the forehead or in the right temple. At that time also he stated that the pain occurred as much, if not more, by day than by night; but subsequently, when his mental state had greatly improved, he was decided in asserting that the pain had been much worse at night. The hemiplegia, he said, had been worse after the fits; the fits began in the eyes, and in them the eyes and head turned to the right. He had been troubled with vomiting. The temperature in the axillæ ranged between  $97^{\circ}$  and  $98^{\circ}$  on several occasions; the viscera of thorax, &c., appeared to be healthy, and, except the temporal periostitis, there were no external indications of syphilis. On the left shoulder was a large cicatrix following the carbuncle of 1873, after which, indeed, he had never regained full power and freedom of movement in the left upper extremity. Mental perception and memory were impaired; he was somewhat childish, demented, and at times in very low spirits. Notwithstanding the absence of any history of syphilis, and of any external sign of it, except the ambiguous one just mentioned, from the above grouping and succession of symptoms, and from the age of the patient, the diagnosis was made of syphilitic neoplasm affecting the meninges and brain on the right side, and he was placed (July 2nd) on twelve grains of iodide of potassium three times a day, in mixture with bromide of potassium, carbonate of ammonia, and Sp. Chloroformi. The obstinate constipation which still existed was met by giving a small dose of black draught in a tumbler of cold water each morning an hour before breakfast. This soon regulated the evacuations, and was never required after the iodide had assumed sway. Three days after commencing the iodide mixture, he had entirely lost the severe cranial pain. On July 7th, five days after the treatment was begun, the pericranial swelling over the right temporal region had also disappeared, and there was some improvement in the motor power of left arm, especially in the shoulder movements. On July 24th, great improvement continued; he walked very fairly for one who had recently become blind, and had regained still more power in the left arm, hand, &c., though of course the grasp with that hand was still weak. There was far less tendency to conjugated deviation of the head and eyes, and, in a word, the hemiplegia was vastly improved. The pupils were a little less widely dilated. Subsequently, he had occasional attacks of epigastric pain, lasting a few hours, and once (Aug. 9th) transitory pain in the right frontal region. By August 29th, the hemiplegia had almost totally disappeared, and

the dose had (22nd) been gradually raised to twenty-four grains three times a day, or seventy-two grains daily. The blindness, however, was virtually unimproved. On September 1st, hypodermic injections of strychnia were begun over the temporal and mastoid regions, and were continued for several months. At first  $\frac{1}{15}$  grain was injected once a day, but the strength of the injection was gradually raised to  $\frac{1}{10}$  grain once a day, and to  $\frac{1}{15}$  grain twice a day. By the end of October the pupils were not totally immobile as they had been on admission, and he thought he could distinguish day from night by vision. He was sanguine about the recovery of his sight, having "glimmers of light," but these were merely subjective sensations. At that date the mental and physical improvement had continued; he had gained seventeen pounds in weight since admission, and for some weeks had been taking a very large amount of exercise, and, at his own strong desire, had been allowed to join the other patients in taking a four or five mile walk. Still there were faint traces of the former left hemiplegia about the face, and perhaps about the left arm, but the energy and freedom of motion natural to that limb had never been fully recovered after the occurrence of the carbuncle. It was, however, slightly cooler, and rather more readily assumed a bluish colour than the right. On testing with a pair of compasses it was found that the tactile sensibility was still diminished on the left side of the face, and slightly so in the left upper extremity, as compared with the corresponding parts on the right side, but he denied having any subjective feeling of numbness in those parts. A slight degree of childishness was observed, and a disposition to laugh on very trifling occasions. Hydrarg. Perchlor., gr.  $\frac{1}{15}$ , was added to each dose.

A slight convulsive seizure took place during the autumn, and another on December 19th, and the bromide, which had been omitted from the mixture since September 5th, was resumed for a few weeks after the occurrence of the last mentioned fit; the iodide, the better anti-convulsive of the two in these cases, being persistently used.

*Remarks.*—He continues strong, hearty, and rational. His memory is now reliable. He states that he suffered from the cranial pain every day from the end of 1873 until after his admission here, that it was located on the right side of the head, and later on was associated with giddiness; with vomiting, from about July, 1874, and "a flare in the eyes"; with insomnia from about September, 1874; and with loss of sight, which began on October 20th of the same year and gradually increased. The pain, he says, was shooting, accompanied by great tenderness, was worse at night, lasted all night, and to relieve it he used to apply coldwater cloths to his head.

A seizure of left unilateral convulsion occurred on February 16th

last, and on March 5th the eyes twitched for a few moments to the left, and the fingers of the left hand closed spasmodically; but the spasmodic action did not spread nor did he lose consciousness. This patient, therefore, has improved marvellously under the above treatment. He has recovered from his dementia and hemiplegia; from the intense osteocopic pain, which had tortured him nightly for more than a year and a half; and from vertigo and vomiting, both of about a year's duration. But his blindness remains and is irremediable, and the horizon of hope for his future health is dimmed by an ominous cloud—the tendency to occasional epileptiform attacks, of which he has had about four during the nine months' treatment here.

### *Résumé of Treatment.*

1875. July 2.—Potassii Iodid. grs. xii, three times a day. Increase on July 30 to grs. xvi. August 22 to grs. xxiv. March, 1876, to grs. xxx, t.d.  
 " " Potassii Bromid. grs. xx, three times a day. Omitted in September, resumed in December. Again omitted, January, 1876.  
 " " Ammon. Carb. grs. vi, three times a day. Sp. Chlorof. q. s.  
 " October 31.—Add. Hydrarg. Perchlor. grs.  $\frac{1}{4}$ , three times a day.  
 1876. February 6.—Increase Hydrarg. Perchlor. to gr.  $\frac{1}{4}$  t. d.

**CASE 6.**—*History of severe secondary syphilis, and of orchitis (syphilitic?) followed by atrophy. Rash; node; insomnia; subse-  
 quently, violent osteocopic pains; pericranial nodes, nausea,  
 vomiting, impaired sight, early optic neuritis.*

Pte. 14th Regiment, age 35, service 18 years, admitted January, 1874. The attack of mental disorder was stated to be the first, to have existed since April, 1873, and to have been "preceded by a long and severe train of secondary syphilis and scrofula." Insanity developed in India, where, and at Netley, he had been treated previously to his admission here. Delusions and excitement obtained from the first, these continued at Netley, and he became impulsively violent towards other patients under the false notion that they came into his room at night and annoyed or injured him. At Netley, also, he had hallucinations of hearing and many of the delusions to be mentioned presently.

*On Admission* here. Height, 5 ft. 10 in.; weight, 10 st. 10 lbs.; dark complexion, a rather dingy pallor of features, lips of good red colour, a suspicious looking papular eruption on the trunk, perios-teal thickening and swelling over the left clavicle, faint trace of a scar on the penis, and inguinal glands large and firm. Traces of old strumous adenitis were found on the neck. There was a scar on the left side of the scrotum, and the left testis was atrophied. In explanation, he said that the testicle was swollen, about two years before, to the size of a fist and was lanced. There were white cicatrices on the abdomen from some former eruption. The pupils



were equal and acted sluggishly. He said he had had "chancres" ten years before, and again, three years before.

He was the subject of a variety of delusions, hallucinations, and illusions. He stated that when he was in India pipes kept blowing his thoughts through his nose, that his eyes were taken out by the regimental surgeon and replaced after half an hour, and that he had been, and still was, constantly tormented. He explained that "men sent their thoughts like voices to plague him;" or, again, "blew their breath against the air to plague thought." He named a former fellow-soldier in his regiment whom he declared to be most active in carrying on this persecution. This man, he said, "blew wind and stuff into his food and into his body, and plagued him all over India, calling him Lord Mayo," and vexatiously dubbing him with other titles; and, on the other hand, personating him. At times he was much tormented by these auditory hallucinations and corporeal illusions, at others much less so; but he complained much of insomnia, of headache and pains about the body, and from time to time was excited and threatening with reference to the subjects of his delusions. He was ordered Potass. Iod. grs. vi, and Hydrarg. Perchlor. gr.  $\frac{1}{15}$ , three times a day. A night draught of morphia gave some relief to the insomnia, but was apt to produce gastric disturbance, and was soon exchanged for chloral hydrate, which proved hypnotic. The iodide was omitted and the mercurial increased to gr.  $\frac{1}{10}$  in each dose, and while he was taking this as well as the chloral at night, the hallucinations became much less distressing for the time. After a course of the mercurial it was omitted, and the iodide was resumed, and given in doses of grs. xii three times a day, together with Ammon. Carb. and Quassia, and the addition of Tr. Hyoscyam. when excitement came on; but in a few weeks he refused the medicine for the time being.

In July he complained much of pain about the right scapular region, had a few scaly patches on the chest, and talked much of tobacco ashes having been blown into his eyes and of the eyes having been taken out under chloroform forcibly administered to him. In truth, there is no trace and no history of any operation. In a few days he agreed to recommence taking the iodide. From time to time, subsequently, he suffered from headache of a severe kind and from pains about the body. He also took the iodide and hypnotics during several periods, but it will be more convenient to append a *résumé* of the medicinal treatment at the close of this recital. The iodide proved of decided efficacy in alleviating or removing the pains. But in his mental state, though there was the very decided mitigation of the more distressing symptoms already mentioned, there was still the delusional element. In February, 1875 he was much excited, and was threatening on the subject of his detention in the asylum. In May the syr. ferri

iodidi was ordered, and was continued by itself. In August, he had a severe attack, in which the prominent symptom was severe shooting pain in the left temple with great tenderness. The pain occurred both by day and by night, underwent remissions, was accompanied by vomiting and slight optic neuritis. The tenderness was greatest at certain points; the pulse was soft, regular, and sixty in the minute. This attack wore away under treatment. In December, there was another attack. In this he had frontal pain and tenderness, and tenderness over the scalp generally. Again the pain was sharp and shooting, occurred both by day and by night, and disturbed his rest, while the delusions became prominent. Two days afterwards there were severe pain and great tenderness over the right mastoid process and right temple. The right temple, it must be said, had been hurt by a pebble struck up by a shell before Sebastopol. In a few days the pericranial tenderness had extended to the right side of the occiput, where there was a large elastic pericranial swelling, vomiting occurred more frequently and was associated with nausea, anorexia, dimness of sight, with a heavy, oppressed, cachectic look, and a soft and rather slow pulse. This attack passed off under large doses of potass. iodid. with syr. ferri iodidi, minute doses of ipecacuan, empl. cantharidis to the nape, and aperients p. r. n. A few weeks afterwards, in the beginning of the present year, it was noted that, physically, he was much improved and the complexion was much clearer; that his memory was good, his delusions far less obtrusive, and his hallucinations infinitely less worrying to him. He talked quietly on the subjects of his delusions and auditory hallucinations. "The thoughts," he said, "sent to persecute people affect them like a dart:"—the darts usually strike *him* on the arms, temples, or back of the head.

*Remarks.*—On various occasions the mitigation or dispersion of the severe cranial pains by specific medication has been sufficiently well marked, and whenever the latter has been discontinued for any length of time the pains have soon recurred; but neither in this, nor in any, case is it desired by the writer to base the diagnosis to any extent upon the influence of iodide of potassium upon either the headache or any other of the symptoms. Iodide of potassium acts beneficially upon certain kinds of cranial pain other than the syphilitic, though upon no others with the same directness and power.

In the above case it was thought during the last illness that the postero-lateral part of the dura mater near the base and in the neighbourhood of the cerebellum was the seat of syphilitic pachymeningitis, either gummatous or simple.

*Résumé of Treatment.*

1874. January 29.—Potassii Iodid. grs. vi, and Hydrarg. Perchlor. gr.  $\frac{1}{4}$ , three times a day, to February 17th.  
 „ February 17.—Hydrarg. Perchlor. gr.  $\frac{1}{4}$ , three times a day, to May 5th.  
 „ May 5.—Potassii Iodid. grs. xii, three times a day, with Amm. Carb., Tr. Hyos. and Quass. to end of June; refused more.  
 „ July 28.—Potassii Iodid. grs. xii, Ammon. Carb. grs. vi, and Ferri et Ammon. Cit. grs. ii, three times a day.  
 „ October 6.—Potassii Iodid. grs. vi, and Ammon. Carb. grs. iv, three times a day. This was taken rather irregularly until April, 1875.  
 1875. May 2.—Syr. Ferri Iodid. 3ss, three times a day, to August 29th.  
 „ August 29.—Potassii Iodid. grs. xvi, and Syr. Ferri Iodid. 3ss, three times a day, to December 6th.  
 „ December 18.—Resumed the mixture of August 29, with temporary additions as above mentioned, and continued it until the present time (March, 1876).

CASE 7.—*Syphilis; palsy of third cranial nerve; hemiplegia; intellectual and emotional weakness; syphilitic periostitis.*

A private in the 66th Regiment, aged 39, of 19 years' service, was admitted in April 1875, having previously been under treatment for mental symptoms in India and at Netley. The attack of mental disease was stated to be the first; it had developed insidiously in 1875, and was attributed to the joint influence of syphilis, intemperance, and tropical climate. He had suffered from constitutional syphilis in 1860-61, and was stated to have had a "fit" in 1861, and symptoms of "paralysis agitans" not long after. In January, 1874, palsy of the left third cranial nerve occurred, and, in January 1875, right hemiplegia. Admitted at Netley early in April 1875, he was found to be openly addicted to onanism which was considered to be an effect, and not a cause, of the psychical decadency present; for he was certified to be of defective perception, memory, and reason; to have vicious and filthy propensities, and to be in a state of utter mental and physical feebleness.

*On admission* to Grove Hall. Height 5ft. 5in., weight, 10st. 10lbs. There was a slight scar on the glans and irregular scars were seen on the inner surface of the prepuce; also a cicatrix from bubo, an irregular scar on the cheek, scattered cicatrices of some former rash on the body, and scaliness about the legs. Slight paralysis existed on the right side of the face; incomplete palsy of the right upper extremity, most marked in the hand, the fingers being stiffly flexed; and an equal degree of palsy in the right lower extremity, the foot being carried very clumsily and planted heavily on the floor, and progression being very slow, difficult and unsafe. He complained of numbness and a pricking sensation in the right arm, and both palsied limbs were at times the seat of severe paroxysmal pain. When he stood for a little while the whole body was agitated by spasmodic tremor. Speech was impaired, mumbling and indistinct, with elision of syllables. He was extremely demented and amnesic, utterly unable to give any correct history of his life; and

exhibited much emotional weakness, weeping whenever he felt the pain and often when no such cause existed.

From time to time he continued to complain of pain in the head and pain in the palsied limbs. The pain often came on when he was sitting, and movement seemed to give some relief to that in the lower limb; but this was not the case with the arm, movement of it brought on pain at the insertions of the deltoid and biceps. In November the mental and physical conditions were much the same, and there were still occasional attacks of pain, especially about the front of the right upper extremity, the outer and back part of the right thigh, and the inner part of the right leg. A few scattered, slightly scaly, papules were seen on the body, and a periosteal node over the right shin with great tenderness. In February, 1876, the notes state that he still had, at times, pain in the head, and sharp and shooting pains about the palsied limbs, worse, he said, at night from time of going to bed at 8 p.m., to midnight. With the pain, also, was tenderness about the right hip and the outside of the right thigh and leg, and there were still some remains of the tibial periostitis. The hemiplegia remained about the same. With the view of giving him every chance of recovery iodide of potassium has been given in full doses, at first of ten, then of fifteen, and finally of twenty-four grains, three times a day, together with carbonate of ammonia, and latterly small doses of perchloride of mercury.

*Remarks.*—The lesions producing the principal symptoms in this case,—the dementia and paralysis,—are no doubt secondary to the intracranial syphilitic changes, and irremediable, therefore, by specific treatment. Though dependent upon a syphilitic process those lesions are not themselves syphilitic. The opinion that the nervous symptoms originated from syphilis, although in an indirect manner,<sup>1</sup> depends partly upon the coexisting signs of present, and the traces of past, syphilitic processes; partly upon the kind of association and succession of the nervous symptoms themselves:—palsy of a third cranial nerve, not permanent; mental alienation; hemiplegia; spasmodic tremor; nocturnal pains. The age of the patient and the absence of any apparent renal or cardiac disease would also favour this view.

CASE 8.—Private in 67th Regiment, admitted in January, 1874, at the age of 35, having served 14 years. The attack of insanity was stated to be the first, to have existed since May, 1873, and it was attributed to the effects of tropical climate. From 1868 to 1870 hospital entries were recorded for dysentery, fever, and diarrhoea. He was stated to be suicidal and dangerous to others, and an attempt at suicide in Burmah was the recognised onset of his insanity, but no doubt the mental disease had been gradually developing for some

<sup>1</sup> On Hemiplegia in Syphilis from Local Softening, see Jackson, 'Journ. Ment. Science.'

time previous to that act. He was treated in Burmah for several months and then at Netley. At the latter place he had vivid hallucinations of sight and hearing, was loquacious and noisy, and became dangerously violent under the influence of delusions such as that the other patients were endeavouring "to eat his arms," etc.

On admission here, there was evidence of ecthyma on the left thigh, and several small punched-out ulcers were seen on the left leg. There was a small cicatrix on the penis and another in the right groin. He said that he contracted the sore in 1864. The aspect was sallow, cachectic, the splenic and hepatic dulness normal. He was incoherent, and abounded in delusions. He said "he had been born three times," "sees his mother now, sees witchery carried on, and has been made to eat human flesh, and to take poison," etc. He continued to be loquacious, excitable, irritable, and at times scowling, threatening, or impulsively violent, owing to the permanency of delusions of a character similar to those above stated. The excitement, however, was partially controlled by treatment. In 1875 he had become much more quiet, and the former delusions of personal injury made way, to a considerable extent, for extravagant and irreconcilable notions as to his personal identity. "He was the Prince of Wales and Napoleon; Anna Maria went about poisoning the floors," and now and then he shouted to have her kept away from him. A large punched-out ulcer of a tertiary type came on the left leg, and in a few days sudden paralysis of the left sixth cranial nerve set in, producing internal strabismus and impossibility of directing the axis of that eye in the horizontal outward direction, and he made some complaint of cranial pain. Under iodide of potassium both the ocular palsy and the ulceration disappeared, but the dull sallow pallor of face remained; the edges of the tibiae were irregular, the dulness of the left lobe of the liver was rather wide, but he was intolerant of minute examination, and gave contradictory statements as to the continuance of any cranial pain.

*Remarks.*—In such cases as this, syphilis, I think, is merely associated with the insanity by accident, and has nothing whatever to do with causing it in the onset. Syphilitic cachexia, like most cachexiæ, occurring in an insane patient would add to the difficulty of recovering reason. In this case the alienation was of an incurable kind, even in the early periods.

CASE 9.—Age 35, service 17 years, admitted January, 1876, for his first attack of mental disease, which was then stated to have been of about fourth months' duration. He had served part of his time in India and Ashantee. There was a history of convulsion, paralysis, and of syphilis, and mention was made of some old injury to the head. A convulsive seizure occurred at the end of 1874, and, subsequently, left hemiplegia and some affection of speech. He recovered to a considerable extent from these symptoms, but

complained very much of cranial pain, went on furlough, and became insane. Prior to his coming here he had been incoherent, and at times noisy, violent, and impulsive, and thought he saw people following him about, and the ceilings cracking and about to fall upon him.

*On admission* the hemiplegia had disappeared, but the gait was heavy, awkward, and slow, the feet being shuffled along the floor without energy or confidence, but he was really able to take a fair amount of exercise. The grasp of the hands was impaired. There was a slight trace of a former sore on the glans, a scar on the prepuce, and another in the groin; the left inguinal glands were somewhat amygdaloid. The shins were irregular, prominent, and rough, and he said he had had pains and swellings in them some years ago, which were attributed to venereal. There was a slight mucous patch on the buccal membrane. Nothing special was found in the thoracic or abdominal viscera. He complained of having had severe frontal pain with tenderness for a prolonged period; a dull heavy pain, undergoing exacerbations and remissions, or leaving him for days together. The cranial pain still continued both by day and night, and he shrank from slight percussion, the tenderness appearing to be greater on the right than on the left side of the head. When questioned about the affection of speech from which he had suffered, he said that at that time "the words were in his head, but he couldn't get them to come away." He wore a dazed and apprehensive expression, and had a cachectic appearance. Memory was greatly impaired, he had hallucinations, and entertained various delusions. He fancied that he was persecuted by people whom he heard, and formerly used to see also. He said they had followed him everywhere, and were accustomed to take his meat and tobacco away, to make the ceilings flake over him, and to put poison in his food, which he asserted he had tasted. He was, and is, a wreck, both mentally and physically, and treatment can do but little for him.

Though there is a distinct history of syphilis in this case, and evidence of syphilis, past and present, yet the proof of the existence of *intra-cranial* syphilis, past and present, or of the dependence of the nervous and mental symptoms upon it, is not altogether convincing. One more case of a doubtful nature will be added, a case which was one of extreme interest, but in relation to which I am unable to satisfy my mind.

CASE 10.—In this case mental derangement was stated to have supervened rather suddenly about two months before his admission here. He had been restless, irrational, had had hallucinations of sight and hearing, and had been subject to excitement alternating with distress. It was also said that since becoming insane, and before coming here, he had been in the habit of exposing himself indecently and masturbating, as well as of passing the evacuations

under him as he sat in the room. When he came here the expression was obtuse and heavy; the tongue tremulous and protruded with a jerk; the gait extremely shuffling, jerky, and swerving from the direct track, and on standing for a short time the whole body was agitated by tremulous shaking. The saliva and portions of his food kept drivelling from the mouth. He was not so fatuous as he looked, but suffered much from hallucinations. He was very carefully watched for a prolonged period and no indications of masturbation were found. He improved both mentally and physically, gaining in weight and in muscular control and steadiness. He was now able to give a clear history of constitutional syphilis with sore throat, &c., and the external traces of venereal sore remained. Several months after his admission the gait became staggering and somewhat ataxic, and there was some heaviness of expression, but no fresh mental symptom. In a few days the condition grew worse, the gait became very staggering and partook of the ataxic features more strongly, and he complained much of giddiness and of dimness of sight. Marked external strabismus supervened in the right eye, with diplopia. He could stand with the feet placed closely together as long as the eyes were kept open, but the moment he closed them he began to sway about and, had he not been caught, would have fallen each time the experiment was tried. At the same time the power of voluntary resistance to passive motion in the legs was so great that it was almost impossible to bend them to the slightest extent against his will. The strabismus partially disappeared, and the diplopia entirely, and hydrarg. perchlor. was ordered in doses of gr.  $\frac{1}{10}$  three times a day. This he took for eleven weeks, the above symptoms entirely passed away, the mental improvement continued, and he was discharged—recovered, four months after the occurrence of the ataxic symptoms.

*Remarks.*—The attack in which sudden palsy of fibres of the right third nerve, vertigo, and dimness of sight occurred, was associated with symptoms simulating progressive locomotor ataxy, but the absence of the characteristic neuralgia and of the several anæsthesiæ of the lower extremities, as well as the rapid recovery, precluded the diagnosis of that affection, otherwise the ocular symptoms would have been attributed to it. I am aware of the deficiencies of this case, and am not disposed to place much stress on the apparent influence of treatment. To strengthen the suggestion that his syphilis may have produced the nervous affections in this patient, and to show that the case is not wholly anomalous, the opinion of Lancereaux<sup>1</sup> may be added here. He says—"To these lesions of medullary syphilis might we not add certain cases of grey or amyloid degeneration which, in a symptomatic point of view, find

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<sup>1</sup> Op. cit., vol. ii, p. 86.

their expression in ataxy of the movement of the lower extremities? Without giving a positive opinion on this point, we cannot refrain from pointing out that ataxy is frequently met with in individuals who have had syphilitic affections, and, under such circumstances, it is allowable to suppose that the syphilitic diathesis may have an influence on the amyloid degeneration of the spinal cord analogous to that which it exerts upon the waxy degeneration of the liver and some other organs."

In other cases under care here intracranial syphilitic processes were suspected—such cases as those in which, with pericranial nodes, there were osteocopic pains and exacerbation of the mental symptoms, or new mental symptoms, and where all these symptoms subsided, apparently in response to specific treatment. How far, on the one hand, the mental symptoms thus brought out may have depended merely upon the element of pain and upon the accompanying insomnia—in the same way as they might have depended upon a common neuralgia, for instance—or, on the other hand, upon a more direct and material action of the poison, or of the lesions caused by it, upon the cerebrum, I will not pretend to say.

In the cases given above gross intracranial organic changes were either found at the autopsies, or were believed to exist where the diseases had not proved fatal. Those have been excluded in which, while organic changes of a milder or more transitory nature than occurred in the above might fairly be suspected, there was, after all, only proof of functional neurosis. Instances in which the question arose of the relation between the secondary syphilis and the mania present will be referred to in the next part.

*(To be continued.)*



II.—On the Association of Urinary Deposits, with remarks upon their probable Causes.—By W. HENRY KESTEVEN, M.R.C.S.

My object in the present communication is to bring forward certain conclusions with reference to the concurrence of, and the pathological and therapeutical significance, of some common forms of urinary deposits, when in excess. These conclusions are based upon the chemical and microscopical examination of deposits from morbid urine, in cases of which I have for several years preserved notes and drawings. The deposits to which I refer are uric acid, calcium oxalate and oxalurate, and ammonio magnesian or triple phosphate.

If we refer to works on physiology we find two, at least, of these substances among the normal constituents of urine, viz. uric acid and ammonio-magnesian phosphate. Of the third, calcium oxalate, although it cannot be said to be constantly found in urine, Dr. Golding Bird has shown that it is often present from various causes during health. In the specimens which I have examined with reference to the present observations, these substances were always in excess.

*Uric Acid.*—That the appearance of an excess of this acid in the urine is caused by a faulty condition of the blood there is no doubt. The difficulty lies in saying in what that faulty condition consists, and where, or in what organ, that morbid condition is brought about.

Uric acid, when in excess, in common with some forms of albumen and with calcium oxalate, is a product of what is called “retrograde metamorphosis”—in other words, of a degeneration of tissue—in contradistinction to regeneration or repair. My conclusion with regard to this question, based on the facts about to be adduced, is that this faulty condition of the blood takes place in the respiratory organs. The blood, from various causes, is not properly oxygenated. In consequence of this vice in its nutrition, the tissues generally are badly nourished, and instead of the urine containing its normal proportion of uric acid, this constituent is in considerable excess. Any circumstance, or combination of circumstances, that prevents the blood from receiving its due supply of oxygen, will produce an excess of uric acid in the urine, *e.g.*, a strumous diathesis, so often associated with tubercular mischief, a weak heart, confinement to the house arising either from disinclination or disability for exercise. Any of these circumstances may be regarded as giving rise to this condition in a greater or less degree. The following facts bear strongly in this direction. In 150 carefully recorded microscopical observations I find twenty-seven in which uric acid was in excess. Of these twenty-seven observations six were taken from the urine of children, three from that of a young pregnant woman, four from

adult men, three from old men, and eleven from aged women. Of the six observations from children, four were from different specimens of urine of the same child, a girl, aged six years, and two were from a boy, of about the same age. Each of these children was markedly strumous. The three observations taken during pregnancy occurred in the same patient, who was suffering from albuminuria. She was, by my advice, staying at home, maintaining a recumbent position, as she had cedema of the feet and ankles. In the fourteen observations from old people the presence of excess of uric acid in the urine was associated either with corpulency and difficulty of breathing, asthma, weakness of the heart's action, emphysema, and in some of the cases from all these conditions combined. In the four observations from adult men three were from a man with asthma and emphysema of the lungs. In the fourth observation in this class I cannot so distinctly attribute the condition to derangement of any one or more organs.

From these observations it is fair to conclude that the main cause of an excessive excretion of uric acid is due to a vicious nutrition of the blood, consisting in a defective supply of oxygen, in consequence of an impairment of the pulmonary function.

*Calcium Oxalate.*—Crystals of this salt are most commonly met with in the form of an octahedron. They are also said sometimes to assume the form of a dumb-bell, but it is now generally admitted that this form is another chemical compound which has received the name of *Calcium Oxalurate*. Among my observations I have thirty-nine in which an excess of calcium oxalate has been found. In twenty-two of these it was the only crystalline substance represented. In fourteen instances there was also uric acid in excess, and in the remaining two there was also calcium phosphate. In nearly every case from which these observations have been taken there has been found also, either at the same time or at some other period, an excess of uric acid in the urine. In many of them, there have also appeared a few dumb-bell crystals of calcium oxalurate. Further, among the specimens above referred to, in which uric acid has been in excess, I have sometimes found these same dumb-bell crystals.

From these circumstances I conclude that in many cases one and the same cause may be operative in producing these seeming differences. This cause, however, is not always equally efficient, and hence the difference in the resulting appearances. Taking for granted that the cause of these is a deficient supply of oxygen, we shall see from the chemical composition of uric acid, as compared with that of oxalic acid, that it is in the production of the former of these acids that this common cause is most potent. The chemical symbol for oxalic acid is  $\text{H}_2\text{C}_2\text{O}_4$ , a

non-nitrogenous substance. That for uric acid,  $H_4C_5N_4O_3$ ; this therefore contains, besides the nitrogen which oxalic acid has not, more carbon and hydrogen, and less oxygen. It is therefore not difficult to conceive that a substance which, like uric acid, contains more of the products of decomposition, should be called into existence when the decomposing cause is more potent, and that a substance like oxalic acid should be formed when the cause is less potent. The substance before alluded to, calcium oxalurate, would seem, from its name, to be an intermediate step between these two substances. The fact that it is found sometimes in specimens in which uric acid is in excess, sometimes where the oxalate is in excess, and lastly, in specimens where both have been found, also bears out this view.

These facts and the coexistence of these substances in the same urine, the occasional substitution of one for the other in urine taken at different times from the same patient, manifestly lead to the conclusion that they are nearly always the result of one and the same cause. That cause, as already pointed out, is a faulty condition of the blood, consisting in a deficiency of oxygen, brought about by impairment of the respiratory function.

With regard to the treatment of the morbid conditions, indicated by the appearances of these deposits in the urine, it is commonly laid down in the text-books that when we have to deal with uric acid in excess we should give alkalis; and such mineral acids as nitric and hydrochloric acids, when we meet with calcium oxalate; and we are further told that what "favours in the first would be detrimental in the second;" that what would be of service against the oxalic deposit is likely to encourage the lithic. The view here taken of the nature of these pathological conditions militates against these rules. The more rational treatment of patients suffering from these morbid conditions seems to consist in the adoption of such means as will obviate or do away with the cause of the morbid condition, that is to say, by ensuring a more liberal supply of oxygen to the blood. As the causes of the deficiency of this important ingredient are many, so the means adopted to obviate them must be numerous. But there is no doubt that one of the most successful methods of treating these complaints is the removal of patients suffering from them to places where they may breath a purer air.

*Ammonio-magnesian Phosphate.*—This salt was formerly called triple phosphate; it appears in the urine in various forms, the most common of which is that of triangular formed prisms, with obliquely truncated extremities. Dr. Beale says—"It is most generally found in urine which also contains phosphate of lime in granules or amorphous masses." Ammonio-magnesian phosphate can be produced in any urine by the addition of a strong solution of ammonia. When thus produced it has a peculiar penniform appearance; this,

however, is only an early stage in the formation of the complete prism. Amongst my recorded observations I find eighteen in which this salt is figured as being in excess. On closer study of these observations I have noticed the fact that the urine from which they were taken was always alkaline, with the exception of one or two specimens, which are recorded as slightly acid. Besides these eighteen observations there are five in which there was found calcium phosphate, either alone or with some calcium oxalate; these are all noted as slightly acid.

There are not many observations in which ammonio-magnesian phosphate and calcium phosphate are recorded as coexistent.

It has been mentioned that it is possible to produce penniform crystals of ammonio-magnesian phosphate in any urine by the addition of strong solution of ammonia; also that these penniform crystals are merely modifications of the perfect prism. These facts, taken with the constant alkalinity of those specimens in which there has been found an excess of the salt, point at once to, at least, one cause of the appearance of these crystals.

Among the normal constituents of urine, is the substance called urea. This, like many other highly complex chemical compounds, is very unstable in its nature. It is readily decomposed, and one of the results of its decomposition is the production of ammonia. The presence of this ammonia causes the formation and deposition of the crystal of ammonio-magnesian phosphate. The formation, however, of the crystals in this way takes place much more slowly, comparatively speaking, than when strong solution of ammonia is added to the urine; and this is the reason probably why the perfect form of the crystal, and not the penniform modification, is found in urine which has been left to itself. The appearance, then, of these crystals in the urine may be brought about by any cause which will detain the urine in its passage from the kidneys a sufficiently long time to disturb the weak connections which bind together the constituents of the urea.

These causes may, of course, exist in any part of the urinary passages—the urethra, the bladder, the ureters, or the pelves of the kidneys themselves. One case which came under my notice deserves attention from its peculiarity. At a post-mortem examination I found the ureter plugged at the upper extremity with a whitish mass, portions of which were also found on the folds and interstices of the lining membrane of the pelvis of the kidney. On examination this was found to be composed of epithelial debris and innumerable crystals of ammonio-magnesian phosphate.<sup>1</sup>

Besides the alkalinity mentioned above there is also recorded in many observations the existence in them of scales of vesical epi-

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<sup>1</sup> 'Pathol. Trans,' vol. xxiv, p. 13.

thelium, and in some also of pus-cells. Although these epithelial scales and pus-cells have been found in other specimens, they are more often found associated with this salt than with any other, and the fact must be borne in mind that their existence would not have been recorded at all had they not been in excess. Their presence proves that there has been in these cases some considerable amount of vesical irritation. Ammonio-magnesian phosphate, as is known to clinical observation, is at times present in the urine of patients suffering from eruptive fevers. This is partly, I apprehend, to be accounted for by the fact that in most illnesses the urine is retained for a longer time in the bladder than in health, and further, by the fact that a larger quantity of urea is excreted during attacks of acute illness. A discharge of phosphatic urine is said to be consequent on several kinds of nervous disorders—hysteria, epilepsy, mania, &c. Having regard to the comparatively large quantity of phosphorus which enters into the composition of nervous matter, there is, perhaps, some ground for the assertion that the nervous discharge which takes place in these disorders is associated with the appearance of phosphorus in the urine.

We should, however, bear in mind the more simple cause of its production, namely, the decomposition of the urea; and although it is possible that a discharge of nerve force over and above the normal amount may give rise to an excess of phosphatic salts in the urine, it must be admitted that the ammoniacal cause is the more likely of the two.

Ammonio-magnesian phosphate is, therefore, different from the other substances considered in this paper. It is not so directly a morbid product as they are. The main significance of it is that it points to the fact of a certain amount of urinary retention; beyond that it does not go, and to find out the site and cause of the retention we must appeal to other symptoms.

Shortly, to sum up our conclusions:—With regard to uric acid, calcium oxalate, and calcium oxalurate, these observations prove that they are often coexistent in the same urine, and that they may be found separately, in different specimens of urine from the same patient. This shows that something more than a chemical examination of urine is required for clinical purposes. That requirement is met by the use of the microscope, and thus its value as an instrument of clinical research is enhanced.

The cases in which the appearances recorded in my observations have occurred, and the relation which these appearances bear to one another, seem to prove that the cause of them is to be found in the vitiation of the blood, by impairment of respiratory function. This being the case, the necessity of some modification of the usual method of treatment of these disorders is indicated. Finally, I conclude from the specimens of urine which I have examined that

the appearance therein of ammonio-magnesian phosphate is not so directly a morbid product as is supposed, or as are the other substances I have mentioned.

In the preceding conclusions I may have been anticipated, as we are told that "there is nothing new under the sun," and as a reference to the literature of the subject will show, but being the results of personal research, I offer them as a contribution to clinical medicine. Whether they confirm or correct previous observations I do not venture to assert ; but I may be allowed the belief that to some it may be a new and useful lesson to be reminded that urinary deposits are to be met by the rules of healthy life, with plenty of ozone and oxygen, rather than by chemical solvents.

## Chronicle of Medical Science.

### REPORT ON SURGERY.

BY EDWARD BELLAMY, F.R.C.S.,

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1. *Clinical Memoir on Osteo-sarcoma, Articular and Peri-articular, and the Difficulties of its Diagnosis.* (Gillette, 'Bull. et Mém. de la Soc. de Chir. de Paris,' ii, 2, 1876).—The conclusions drawn by M. Gillette at the close of an elaborate article of this subject are as follows:—1. That the diagnosis of articular and peri-articular osteo-

sarcomas often offers, on account of their insidious progress, very great difficulties, since they may be confounded with osteo-periostitis or with white swellings. 2. If a white swelling has an unusual course, the surgeon should suspect it, and examine if the reunion of a certain number of phenomena which appear abnormal do not cause him to modify his diagnosis. 3. At the onset the diagnosis is almost impossible, the acute pain suggesting neuralgia, rheumatism, or commencing arthritis. 4. These cancerous joint diseases may appear like white swellings in youth, in lymphatic individuals, scrofulous, syphilitic, and their origin be referred, rightly or wrongly, to an injury. 5. Osteo-sarcoma may be suspected, and recognised by the continuous and rapid course taken by the affection, by the acute and persistent pain, in spite of rest and immobility, by the rapid development of the swelling and the special character the masses have, of pushing aside and lifting up the tissues, by the absence of suppuration, or at least by the little tendency these tumours have to suppurate, by their absolute resistance to therapeutic agents, and particularly by the increase of pain following compression, by the integrity of articular movements, which is due to the preservation of the diarthrodial surfaces, and by the absence of any anomalous position taken by the patient, as is the case in white swelling, and by the exploratory puncture. 6. The treatment consists in amputation high up or in disarticulation.

2. *Complete Syphilitic Atresia of the Pharynx.* ('Gazette Méd. de Paris,' Mars 14, 1876.)—M. Mauriac, at the close of an interesting course of lectures on Pharyngo-nasal syphilosis, narrates a case, which from its extreme rarity, is worthy of publication. He states that up till the time of the termination of his lectures, no such case had presented itself to him. The patient, a man, æt. 33, had had an infecting chancre in 1864, followed by several severe attacks of pharyngeal syphilis. In 1865 he lost a considerable part of the velum palati, followed by complete adherence of the posterior wall of the pharynx to the remains of the velum and isthmus, cutting off all communication between the posterior nares and the bucco-pharyngeal cavity. The summary of the case in M. Mauriac's words is as follows:—"The anterior third or half of the velum was affected by the ulceration, and was united to the posterior wall of the pharynx, which was itself the seat of a considerable lesion, as a median cicatrix showed. The result of which was that the palatine arch was prolonged horizontally to the pharynx, and that a *membranous diaphragm*, formed by the *débris* of the velum, divided the pharyngeal canal into two parts, one superior, in which were the posterior nares and the Eustachian tubes, and an inferior or bucco-pharyngeal space. This inferior compartment, situated below the palatine diaphragm, was itself subdivided into two parts by an *incomplete diaphragm*, which was nothing more or less than a new isthmus faucium. It was in a plane directed obliquely backwards from the sides of the tongue to the pharynx, where it became united with the horizontal palatine diaphragm. This septum was formed below by the base of the tongue, and



laterally by two *large folds* of the mucous membrane, which represented the anterior pillars, stretched, widened, and rendered immovable owing to their attachment to the pharynx. It was pierced at the centre, by a triangular opening, the apex of which corresponded with the middle portion of the posterior wall. The portion of the pharynx, situated above the *isthmus diaphragm*, is the posterior nares, that below, the pharynx, which communicated by the triangular opening, so that deglutition was not much hindered. This patient had been treated with iodide of potassium, and there appeared to be no antecedents of scrofula. M. Mauriac did not consider any surgical proceeding was indicated, indeed he thought that it would be worse than useless.

3. *Adhesion of the Free Border and Postero-superior Aspect of the Velum Pendulum Palati and the Pharynx.* ('Proc. Soc. de Chirurg. de Paris,' Ap. 1876.)—M. Verneuil brought forward the following case. The patient was a young woman, married, 22 years of age, who had contracted syphilis since her marriage, and about a year afterwards had suffered great loss of substance of the soft palate, which resulted in a nasal tone of voice, and painful and imperfect deglutition. After a while the projection of the velum posteriorly became evident, and there were all the signs of closure of the posterior cavity of the nasal fossæ. The patient could not blow her nose, or breathe unless the mouth was open; she had intermittent deafness, and examination showed that the velum was completely fastened to the pharyngeal walls. M. Verneuil decided to operate, and proceeded thus—The patient was anesthetized, and the channel which remained between the pharynx and nasal fossæ was enlarged by a knife, a pair of polypus forceps was then introduced, and their blades smartly opened, whilst the lateral adhesions were broken down by means of the fingers. He then placed between the velum and the pharynx an india-rubber apparatus, consisting of two lateral tubes, and of a series of transverse smaller ones, the anterior openings of the larger tubes passing out through the nostrils and the posterior by the mouth, opposite to the labial commissures, but it was found necessary to remove the instrument, as after some days specific ulceration showed itself wherever it was in contact. M. Verneuil had then recourse to progressive dilatation, by means of a sound ending in a caoutchouc bag, which was introduced by the nostrils and inflated; by repeating this proceeding daily it was hoped to prevent adhesions forming, but the negligence of the patient frustrated the perfection of the idea, nevertheless a sufficient aperture was formed, by means of which the patient could breathe and blow the nose, the sense of smell returned, and the nasal character of the voice was noticeable only during rapid utterance and in a loud tone. During the subsequent discussion M. Lucas-Championnière remarked that on a similar occasion he endeavoured to introduce a hollow sound from behind forwards, but after two hours' attempt, being unable to do so, he made lateral incisions to effect his object, but, notwithstanding the employment of india-rubber laminæ, adhesion still took place. At a second operation, he cut down with a single stroke into the nasal

fossæ behind the velum. The wound healed, but the patient has been forced to wear a silver tube through the nasal fossæ. As M. L.-Championnière remarks very truly, surgery has not had its last word on this question, and until the problem is solved, it is indispensable to have recourse to appliances to prevent the production of these adhesions.

4. *Revolver Bullet Wound of the Heart; recovery.*—Dr. E. Anders describes the following case, which occurred in the Clinic of Prof. Bergmann, of Dorpat ('*Deutsche Zeitschr. f. Chir.*,' vi, 1 and 2, p. 191, 1875). The diagnosis was arrived at on anatomical and physiological considerations, and the case, on account of its rarity, is of great interest. (G. Fischer, in his work on wounds of the heart, mentions only twelve cases.) A young man, in an attempt at suicide, shot himself in the region of the heart. After the original dyspnoea and hæmorrhage had ceased he was able to take the journey to Dorpat (fifteen miles). On admission the wound, which was round, about as large as a sixpence, was already scabbed over, corresponded exactly with the heart's impulse, and exhibited rhythmic pulsations. The temp. was  $32.2^{\circ}$  C., resp. 42; there was then dyspnoea, anxious expression, and vertigo. The area of the heart's dulness began at the upper edge of the third rib and extended to the edge of the sternum at the right side, and outwardly on the left to one centimètre above the line of the nipple. On the left side, behind and below there was dulness, no pectoral fremitus, but a pericardial friction sound at a circumscribed spot at the level of the fourth intercostal space. During the next two days the irritation of coughing was stronger, pulse frequent; evening temp.  $39^{\circ}$ . On the fourth day the dulness had risen on the posterior wall of the thorax, to the angle of the scapula, and in the upper zone there was pectoral fremitus and bronchial breathing, and the heart's dulness had extended outwards to one centimètre above the right edge of the sternum. There was pericardial exudation, and the left lung was the seat of pulmonary mischief. On the fifth day the pericardial exudation had increased outwards four centimètres over the right side of the sternum, and there was great sweating at night. From this time, however, the symptoms began to disappear. The exudations became absorbed rapidly and on the tenth day after the receipt of injury the heart's dulness reached only as far as the middle line of the thorax, and the pericardial rub had gone. Twenty days after the injury the wound had healed and its cicatrix moved synchronously with the systole. The examination of the lungs showed them to be tolerably normal and the absolute heart dulness reached only to a finger's breadth beyond the left edge of the sternum. The impulse during systole below the left nipple was clearly to be made out in the fourth intercostal space, and feeble in the fifth. The first sound was divided, this symptom was weakest over the aorta, and ceased in the recumbent position. The point of pathological importance in this case is the behaviour of the heart's beat. Anomalous conditions of the circulation were not observed, and thus a perforation of the walls of the heart, with escape

of blood into the pericardium was to be excluded from the diagnosis, although a wound of the wall of the heart may have been present, as is learnt from other cases. There was inflammation and exudation tissue: and the existence of the pericarditis is of great importance, which was readily detected at the seat of injury. The anatomical position of the wound afforded the surest datum, being at the normal position of the apex; whilst its direction was such that it must clearly have involved the heart, the nature of the hemorrhage indicated a wound of that part of the lung covering it, since the space, in which the pericardium is united with the anterior wall of the chest, by means of portion of areolar tissue, is so small and lies nearer to the mesial line, so that it could not have been struck. A wound of the pericardium alone is not admissible; it is an extraordinarily rare occurrence, and from the direction of the missile not very probable. The supposition that the bullet, without opening the pericardium remained fixed in the chest wall, or between it and the pleura or pericardium is not tenable, for the results of such a condition were untenable. The drawing in of the cicatrix in the chest wall synchronously with the heart's beat confirmed the diagnosis of a wound of that organ, which in any case had its locality in the neighbourhood of its apex.

5. *Excision of the Astragalus.*—In the 'Medical Examiner' a clinical lecture by Professor Wood, of King's College Hospital, is published on two cases of *excision of the astragalus* for caries of that bone leading to suppuration in the ankle and the calcaneo-astragaloid joints. The cases were of a kind which would have called for amputation of the foot, save for the intervention of a less mutilating and conservative operation. The subject of one was a young man of 19, and of the other a lad of 12 years. They presented a similarity somewhat remarkable in appearance. There was a large pulpy swelling below and behind the inner malleolus, with two or three sinuses leading down through the sheath of the *tibialis posticus* muscle to carious bone, causing swelling of the ankle-joint under the extensor tendons, and accompanied by great pain.

The same kind of dropping of the great toe was seen in both, and in both the constitution showed signs of becoming affected.

The peculiarity of the operation, adopted in these cases for the first time, was in its being performed on the *inner side* of the ankle by opening up and connecting by incision the sinuses already formed. The incisions extended in a curved direction, from the tendon of the *tibialis posticus* to that of the *tibialis anticus*. The interposition of the former tendon was found sufficient to protect the posterior tibial vessels and nerves from exposure and injury. There was found also in this incision room enough for the complete and, in the first case, the *entire* removal of the astragalus. The internal lateral ligament being completely divided, the scalpel was introduced flatwise between the astragalus and calcaneum, as in using an oyster-knife. The interosseous ligament was thus easily divided. The posterior and anterior ligaments were then cut, keeping the knife close to the astragalus. Then the head of the bone was seized by a small pair of

lion forceps, and the whole bone, twisted round on its axis, was brought out of the wound head first, the foot being at the same time powerfully wrenched outwards, so as to separate as much as possible the inner malleolus from the os calcis.

The anterior and posterior fasciculi of the external lateral ligament were by this means torn off from the astragalus, and the bone extracted without difficulty. A counter-opening was then made between the tendo Achillis and the tendons of the peronei muscles, and a large drainage-tube passed through the wound. Pus was found in the ankle and other joints in both cases.

In the latter case a portion of the articulating surface of the calcis was found diseased and gouged away.

The first case made a good recovery, with perfect use and free motion of the ankle-joint, and with only three-fifths of an inch shortening of the limb.

The second case is still under treatment in King's College Hospital, doing well, and promising an equally good result.

A third case, very similar to these, is now under treatment by Professor Wood at the Hospital. (June, 1876).

6. *On the value of Subastragaloid Amputation* ('Bull. Gén. de Thérap.' October 30th, 1875).—In order to obtain a stump which realises the best conditions, both as far as regards the relative rapidity and regularity of the cicatrisation and the power of resisting pressure, sufficient amplitude should be given to the flap, so that the head of the astragalus be easily covered in anteriorly, not merely by the soft parts of the dorsal region, which are not sufficiently protective, but by the skin of the sole. Under these conditions the line of the cicatrix, is protected equally, externally and in front, by the relief which forms the edge of the plantar flap.

The proceeding adopted by M. Perrin differs from that described by Verneuil.

The operative proceedings are as follows:—The first incision commences behind and externally over the insertion of the tendo Achillis, and is directed forwards along the external aspect of the os calcis, passing horizontally three centimètres below the plantar aspect of the external malleolus, terminating at the posterior extremity of the fifth metatarsal bone. From this point the incision is conducted obliquely forwards over the dorsum of the foot, so as to reach its internal plantar border, at the point of articulation of the first metatarsal bone with the internal cuneiform; next it is directed across the sole so as to rejoin the external incision about two centimètres behind the fifth metatarsal bone; the incision thus formed is racket-bat-shaped.

There is here an abundance of the soft internal plantar parts, which cover very amply in front the head of the astragalus, and form throughout the entire extent of the cicatrix a protecting pad against the effects of lateral motion and pressure. The remainder of the operation is performed as directed by Verneuil.

It should be especially noticed that as much as possible of the periosteum be left adherent to the os calcis, so that there may be no

fear of the formation of isolated osteophytes in the flap, which would be liable to cause great pain, and a persistent hindrance to walking.

7. *The Treatment of Hypospadias.*—During the past session Professor Wood has shown, at the Medico-Chirurgical Society and King's College Hospital, three cases of *hypospadias* of a severe character, which had been operated on by his new method of transplantation of the prepuce. In two of the cases the urethra opened at the base of the penis in front of the scrotum, and in the other about two thirds of the way down.

In all the prepuce was tolerably large, crowning the glans penis like a hood, split below, and without frenum. The method consists of turning up two side flaps, one on each side of the groove which represents the urethra, so as to denude the entire lower surface of the penis, and to remove the contracted cicatricial fibrous tissue which acts so unfavorably in producing a downward curve of the organ during erection. These flaps are turned with the skin surface over the urethral groove and the edges stitched together by a continuous suture of fine wire, the ends of which are left untwisted and protruding.

A transverse button-hole aperture is then made through the prepuce on the dorsum of the penis at the attachment to the *corona glandis*. The glans is then passed through the opening thus made.

The two layers of the prepuce are separated by dissection, so as to permit of the raw surface being laid upon that of the reflected flaps which cover the urethra. The prepuce is finally kept in position by numerous points of thin wire suture.

The patients thus operated on were unable previously to pass the water in a jet, and suffered much inconvenience from the dribbling of the urine over the scrotum, down the legs, and over the clothing. They can now pass the water in a full stream from the end of the newly-formed urethra, the lower part of which is formed by a spout-like projection of the transplanted prepuce.

Professor Wood has now in the hospital (June, 1876) a very extreme case of urethral fistula, in which, from the consequences of a horse accident, upwards of two and a half inches of the lower wall of the urethral had sloughed away. After three operations (one rendered abortive by an attack of erysipelas), the opening has been entirely closed in by a similar operation of transplantation from the scrotum and penis.

8. *Femoral Aneurism cured by Direct Compression, while the Patient was taking Active Exercise.* ('Boston Med. and Surg. Journ.,' Oct. 21st, 1875).—This singularly rare case was under the care of Dr. Brown, surgeon to the house of 'The Good Samaritan,' Cambridge, U. S. The closure of the vessel, although commenced when the patient was recumbent, progressed and was completed while he was taking active exercise, and attending, for the greater portion of the time, to business.

The symptoms commenced in 1863, and the diagnosis was aneurism of the femoral at its exit from the abdomen. Treatment by immediate pressure was decided on, and carefully varied weights, from a bag of shot of ten pounds, alternating with ice, to a twenty-four pound shot, were used also in alternation for some eight weeks, with the effect of rendering the pulsation less forcible, of lessening the tumour, and of hardening its parietes, which became comparatively inelastic, whilst the artery below the aneurism was evidently diminished in calibre.

This treatment was continued, with little variation, from October, 1863, to June, 1864. The artery below the aneurism became extremely small and its pulsation scarcely perceptible. The swelling had much diminished in size, had become hard, and its action comparatively feeble. It was decided to continue the treatment which had thus far been attended with so favorable a result, but to apply pressure in another form, and, if possible, in such a manner as to admit of locomotion. A wide, strong, firm leather belt was made, thoroughly padded, which was fastened tightly around the hips; to this was attached a strap passing from behind the trochanter to buckles over Poupart's ligament. A pad was adapted to the tumour, hard, oblong, and convex, with a block-tin back. This pad was held in position by the strap passing through loops to the buckles. These straps having been adjusted, the patient was allowed to sit up and walk a short distance each day. At first his legs were very weak; he rapidly gained strength, however, and was soon able to walk out, and in September, 1865, he began to attend to business. The pad was so accurately adapted to its intended position, and so firmly held there, that motion of the joint did not displace it, and thus a strong pressure upon the tumour was insured, even during active exercise. He continued to wear the belt and pad night and day, never removing it, except when in the horizontal position, and then only for a few moments, for the purpose of bathing the part or to dress the excoriations produced by the belt upon the hips. On an examination, some time since, the artery below the swelling could not be felt, having, so far as could be ascertained, become obliterated by the constant pressure. The tumour pulsed feebly, had become harder, and had little elasticity.

About two years after leaving off the compression pad to the date of death, the patient had no inconvenience from his imperforate femoral. Peritonitis came on suddenly one day and proved fatal in a week. A post-mortem showed no indications that there was any connection between the old arterial disease and the acute complaint.

The ultimate success which attended the treatment of the aneurism was undoubtedly due in part to the fact that the compression at first was not sufficiently forcible to entirely occlude the artery, but was such as gradually to diminish its calibre and to allow of the progressive enlargement of the neighbouring vessels and the accommodation of the surrounding parts to the new state of things. Likewise the process of nature in producing a spontaneous cure was by this course more strictly imitated.

9. *Villous Cancer of the Bladder in the Male.*—Prof. Kocher of Bern ('*Centralblatt für Chirurgie*,' April 1, 1876) says that papilloma vesicæ in the female has been frequently treated by operative proceedings and brought to a satisfactory conclusion, and indeed been healed. Since Simon was shown the proper manner of dilation of the urethra, it has seemed that the advantage derived from the median operation, might be made of use in an analogous manner in the male bladder. Volkmann speaks very highly, in his '*Beiträge für Chirurgie*,' of the median incision for the removal of calculi, so also does Dolbeau in his article '*Lithotripsie périneale*.' The method, however, has as yet not been applied to the removal of new growths, hence the report of the following case. A gentleman, thirty-eight years of age, began to suffer in the spring of 1874 with constant desire for micturition and incontinence of urine. In June of the same year, these symptoms changed to a violent burning pain and an increased desire to pass water. The urine was turbid, with a sediment of pus and blood, and had an "intolerable" smell. The symptoms increased, and in October after two severe rigors, examination by the sound and subsequent microscopic investigation of the withdrawn material showed the presence of a villous growth. On December 31st, the urethra was opened by means of a T-shaped incision, with a small vertical cut in the mesial line and a horizontal one after Nélaton's prerectal method. By this incision, the bulb and the hæmorrhage resulting from its division are avoided. The urethra was opened on a grooved staff and the finger introduced into the bladder. On its posterior wall was felt the soft, tufted, fungoid mass of the new growth, a long sharp scoop, bent at an angle, was introduced against the finger, whereby it was forcibly scraped off. The hæmorrhage was pretty severe, but was arrested by cold injection. No dressing was applied. The patient made water spontaneously, and passed it by the wound the first day. The wound healed in six weeks and in fifteen months the patient was considered as quite cured.

10. *On the Treatment of Deep-seated Atheromatous Cysts of the Neck.*—Esmarch, in the '*Archiv f. Klin. Chir.*,' Bd. 19, b. 2, 1876, has a paper on this subject of which the following is an abstract. The extirpation of these deep-seated atheromatous cysts, on account of their numerous attachments with the sheaths of vessels, is both difficult and dangerous, and in all cases, leaves behind it a considerable scar. Prof. Esmarch exhausts the cyst with a fine trocar, carefully washes out the cavity with a one per cent. solution of carbolic acid, and then injects ten or twenty grammes of Lugol's solution (iodine and iodide of potassium ana 1.25, to 30.0 of water) which is let out after a few minutes. The cyst quickly fills up, is at first somewhat painful, and then shrinks up into a small knot. Should this shrinking-up not take place after six or eight weeks, the operation is to be repeated ('*Centralblatt f. Chir.*,' April 1, 1876).

11. *On Fractures of the Lower End of the Humerus and Femur. Experimental Investigations by Dr. Madelung, of Bonn* ('*Archiv f.*

Klin. Chir.,' B. 9, h. 2, 1876).—The author found by actual experiment on the dead body, that the frequently occurring T- or Y-shaped fractures of the lower end of the humerus, are readily produced by a blow on the bent elbow. Besides both these forms, a chipping off of one of the condyles, with a larger or smaller portion of the end of the articulation happens. Longitudinal fracture commences in the first instance, by the driving of the articular surface of the olecranon into the trochlear surface of the humerus, and the transversely directed portion of the fracture is so brought about, that the wedge-shaped olecranon frequently breaks off only one condyle, with the corresponding piece of the articular surface, and the narrow bony strip binding the other condyle to the shaft breaks subsequently by means of the onwardly acting force.

Dr. Madelung, should a patient after a fall or blow upon the bent forearm come before him, and present the exclusive symptoms of a transverse fracture of the lower end of the humerus above the joint, is, nevertheless, inclined to diagnose the existence of an equally distributed longitudinal separation of both condyles, and a consequent opening of the articulation. The existence of any remarkable amount of blood effused into the joint after a fall on the olecranon with the arm bent, would suggest the separation of a condyle. The fact of the peculiar form of production alone, would, as he believes, after his researches, justify such a diagnosis, also if no mobility of both condyles on each other could be made out or if crepitation and pain be felt on motion. In like manner Dr. Madelung produced the corresponding fractures of the femoral condyles by means of blows upon the patella.

[This paper is of great value as a contribution to experimental research into the real cause of fracture and the conditions produced thereby, and will well repay reading in the original. Investigations of this kind are common enough abroad, but presumably our want of material, renders them rare in England. (ED. REP.)]

12. *On the Curative Treatment of Varicose Dilations of the Superficial Veins of the Limbs, and of Circocoele, by the method of the simple isolation of one or more points in the course of the vessels* (by Dr. Rigaud, of Nancy, 'Soc. de Chirurgie,' Mai 28, 1875).—This method, according to the author, is exempt from the dangers attendant on the several operations for some time since practised on the veins. In a former series of cases, before the year 1852, he cauterised the veins with Vienna paste applied to the skin, then, abandoning this method, he isolated the varicose vein itself, so as to subject it to an even and direct cauterisation. Having had some mishaps by these proceedings, he came to establish definitively the method of *Simple Isolation*. By this method he has isolated the varicose mass entirely for about four or five centimètres of its course, and simply exposed it to the air, whereby it becomes obliterated, dried up, mummified, and very often ruptures spontaneously. He has never seen any phlebitis, properly so-called, following the treatment. Two or three times he has observed lymphangitis, which has been arrested by treatment. The author believes firmly in the innocuousness of his



method, at least as far as he has gone. He cites, it is true, three unfortunate cases, but in these instances the veins had been wounded by the surgeon. But all the danger of operations practised on veins lies in the event of traumatic lesion, and it is as a means of avoiding this that one must essentially value the method of simple isolation. M. Rigaud has practised his method since 1852 for the obliteration of varices of the limbs more than 140 times, and on the veins of the pampiniform plexus, 11 times.

*Experimental Researches on the Mechanism of the Coagulation of the Blood in the Treatment of Varices by the Simple Isolation of the Veins.* (M. A. Bergeron, 'Comptes Rendus Acad. des Sci.,' 26th Oct., 1875).—The object of these researches is to show what occurs in veins thus denuded according to the method of M. Rigaud. The isolation of the vein destroys the cellular envelope, in which are the *vasa vasorum*. First the external tunic, and then the middle tunic sphacelate, and finally the internal tunic and its endothelium; the blood which up till this time had continued to circulate, as it found in its path a regular epithelium smooth and absolutely normal, becomes coagulated on contact with the altered internal tunic, the necrosis of which renders it a foreign body.

13. *Removal of a round-celled Sarcoma of the Pharynx by subhyoid Pharyngotomy* ('Centralblatt für Chirurgie,' No. 3, 1876).—Rosenbach gives the following historical notice of the operation of subhyoid pharyngotomy, that Vidal de Cassis and Malgaigne proposed it, and Langenbeck practised it on the living body, and introduced it into surgery. Langenbeck communicated to the 'Berlin Klin. Wochensch.,' 1870, five cases of this operation, and since this period only one similar operation for the removal of a foreign body has been published by Dr. Leffert. The case under notice was that of a man, forty-five years of age, a forester, who it appears had had symptoms of the development of a tumour, which caused annoyance during the processes of swallowing, speaking, and respiration; sometimes provoking suffocation, choking, or vomiting. Upon one occasion a piece of the growth was brought up, followed by temporary relief. On admission to the clinic, Professor Baum demonstrated a tumour cropping up over the back of the tongue, and obviously necessitating tracheotomy.

During an attempt to remove the growth by Muzeux's forceps, a piece was broken off, which was recognised as a round-celled sarcoma. Before the operation proper was commenced a tampon canula was introduced, and a control apparatus for the safety of the tampon was applied.

The patient was narcotised, and placed on his back so that his head hung downwards. An incision nine centimètres long was made along the under edge of the hyoid bone, and the ligamentum thyrohyoideum medium, and the sterno-hyoid and sterno-thyroid muscles divided. The mucous membrane was drawn into the wound by means of a silver catheter introduced into the pharynx through the mouth, the front of which lay in the fold between the back of the tongue and the epiglottis; the membrane was next cut through, and

the pharynx thus opened, the hyoid bone drawn up and the epiglottis down. The soft ragged mass was torn off by the finger; the hæmorrhage was very free, but in no way interfered with the respiration; the remaining part of the right side of the tumour was ligatured, and the rest removed by scissors. After closure of the wound by deeply-applied sutures their ends were brought out through the mouth. The canula remained in with the inflated tampon, and was removed in eight days; the speech clear and good, breathing free, and rapid healing going on, with no feverishness.

14. *On the Treatment of Amputations by the Open Method.* By F. S. Dennig, M.D. ('New York Med. Journ.,' Jan., 1876).—The writer, who is the house-surgeon of the Bellevue Hospital in New York, in a very careful and interesting paper, records the treatment of amputations by the above method, as carried out in that hospital, where it was introduced by Dr. James R. Wood, one of the surgeons thereto. He commences by a description of Dr. Wood's method of dressing stumps in all its details, and refers to the great advantages that this treatment possesses over that of closing the stump, then replies to a few of the most prominent objections raised by surgeons who are opposed to this manner of treating amputations, and finally publishes the individual histories of a few cases illustrating the great benefits of leaving a stump open to the air. The cardinal principle involved in this method of dressing is that of preventing suppurative fever, and this object is best attained, as is shown, by leaving the stump entirely open, thus allowing of free and continuous drainage. After a limb has been amputated, the flaps are not even approximated, but left entirely open. A pillow of oakum is placed under the stump, which is allowed to rest upon this support until the wound is nearly healed. A small piece of gauze is placed over the contour of the stump, and a cradle is placed over the limb, so that the clothes may not come in contact with the painful extremity. This is all the dressing that is employed; no sutures are used except in the lateral skin-flap method. No adhesive plaster is employed, no oil-silk is placed over the stump, no bandage is applied, no dry charpie is stuffed into the wound, no fenestrated compresses are placed between the flaps; in other words, the stump is left entirely alone, just as the surgeon made it in his amputation. The wound is thus allowed to drain freely, and the stump is gently washed at frequent intervals by means of an Esmarch's wound-douche. The water in this irrigator is impregnated with crystals of carbolic acid, and, after this ablution, balsam of Peru (which makes a fine stimulating application) is poured over the granulating surface. The discharge which falls from the wound is removed every few hours in order to secure perfect cleanliness; and it is a fact worthy of observation that this discharge will not decompose when exposed to the open air, but that it requires a warm temperature, such as exists in the stump itself, in order to develop putrefaction. The pus, thus coming away from a nidus of putrefaction which would otherwise be formed, falls upon a piece of sheet-lint where the temperature is cooler, and thus does no harm. The stump is then washed at fre-

quent intervals until suppuration has nearly subsided in the wound, and then the flaps are gradually approximated by means of strips of adhesive plaster. Too much importance cannot be attached to this method of operating by the lateral skin-flaps. It affords the best facility for free drainage, and makes the most serviceable stump. It is important to dissect the flaps very long when they are subjected to the open treatment, as shrinkage often follows exposure to atmospheric influences. Esmarch's elastic bandage has been employed in every case, and in no instance has sloughing, or any other complication, occurred. The stump after a week is capable of being moulded into any shape which the surgeon's taste may suggest. During the entire healing of the wound the greatest possible care is exercised in reference to the use of the instruments necessary to perform the dressing of the stump. No sponges are ever used in the wards. Each patient has his own bottle of balsam of Peru, and every instrument used in the dressing of one stump is thoroughly washed in carbolic-acid water before it is employed in the dressing of another. So far as has been practicable, a different set of scissors, dressing-forceps, and other instruments employed in the manipulation of a dressing, has been used, so that each patient had his own instruments, and in this way absolute cleanliness is secured. Each dresser invariably washes his hands in carbolic-acid water after dressing one case before undertaking another, and any one who is dressing unhealthy wounds in the pavilion, or making autopsies, is not allowed to even assist in the daily dressing of healthy wounds. Now, the great advantages that are claimed for this method of treatment of stumps, by leaving them entirely open to the air are—

*First.* That suppurative fever is very much modified; indeed, it is almost obviated. In leaving the stump open, and allowing the effete material free drainage from the surface of the wound, the most important factor which produces secondary fever is eradicated, the patient escapes the severe constitutional disturbance dependent upon the absorption of decomposed pus from a closed wound, and the discharge, falling below, where there is a cooler temperature, is at once disarmed of its poisonous character.

The *second* advantage that is claimed for this peculiar treatment of stumps over other methods that are adopted is, that it prevents all possibility of the formation of abscesses in the vicinity of the stump, which are so apt to form in closed stumps, even when they are most carefully watched. In no case that has been subjected to the open treatment has there occurred an abscess, either in the immediate vicinity of the stump, or in any part remote from the wound. The reasons for this are obvious. The conditions that are necessary to develop an abscess are wanting. There is no opportunity for pus to collect within circumscribed limits, there to decompose, and in this way assist, as it must and does, in the formation of an abscess. This is all prevented by a free and constant drainage from the suppurating wound.

Another great advantage that may be claimed in connection with

this practice is the absence of erysipelas in the wound, or in the cellular tissue in the vicinity of the stump.

15. *On the Treatment of Elephantiasis Nasi by decortication* (M. Ollier, 'Lyon Médical,' tome xxi, Mars, 1876).—In cases where the size and thickness of the mass require complete ablation, decortication by means of the knife has a number of inconveniences, such as erysipelas, and more particularly hæmorrhage, and that not only at the time of operation, but even ten days or a fortnight following the cicatricial process, since the diseased masses themselves are highly vascular, and the periosteum and perichondrium are perforated by vessels difficult to tie, and even if directly closed by clot there is great danger of secondary hæmorrhage. M. Ollier therefore used a small cutting cautery heated to white heat, and with great care made the required dissection, with one finger in the nose of the patient, in order to appreciate the thickness of the tissues. Latterly he has used the galvanic cautery (or galvanic knife), as at will one can either augment or decrease its capabilities as a cutting or hæmostatic agent. He calls particular attention to the management of the resulting denuded surface, it being an advantageous course to leave, when possible, small islets of healthy tissue, whilst the tension of the cicatricial process will have the effect of stretching them, and thus render them of use in covering the organ. The aperture of the nostrils should not be interfered with, and a small zone of skin should be invariably left, even at the risk of an ugly-looking pad, as the retraction will sooner or later involve it, and regulate the size of the nostrils. In dissecting off the tumour, great care must be taken in the management of the perichondrium. The process is difficult and laborious, but it is better to leave a layer of that membrane than to denude the cartilages, as they will necrose at the points where it has been removed. Too great retraction of the nostrils, if threatened, may be combated by judicious plugging each aperture separately.

16. *Probably a unique case of Operation for Strangulated Inguinal Hernia, performed forty-five hours after Birth, followed by Recovery of the Patient and a radical cure of the Rupture* (F. Woodbury, M.D., 'Philadelphia Medical Times,' December 25th, 1875).—The operation for the relief of strangulated inguinal hernia by the indirect descent was performed in this instance, perhaps, for the first time on record, upon a patient of such tender age, the infant being less than two days old at the time it was resorted to, after repeated attempts at reduction by taxis and treatment had failed. The case was then in such bad condition that an unfavorable prognosis had been given, and no hopes held out of success.

At birth the child was asphyxiated, and some difficulty was experienced in establishing respiration; but after this it continued to cry and fret, giving the mother the impression that it was in pain; about midnight she noticed a prominence in the right groin, which seemed tender, and then about the size of a small walnut. The following morning the surgeon discovered a tumour, which had now attained the size of an orange, and which he recognised as

a hernia. Taxis was tried ineffectually, and repeated in a couple of hours, after a warm bath and small doses of opium, but with similar unsuccessful result; by this time, however, the swelling was as large as the child's head, and tender and discoloured. A final attempt at reduction was made, but in spite of careful manipulation it again failed.

Ether was now administered, and, just forty-five hours after birth, the operation was performed. Upon opening the sac an indirect inguinal hernia was discovered, which had become strangulated at the internal abdominal ring. The band of tissue causing the constriction was nicked with a probe-pointed bistoury, the opening enlarged with the forefinger, and the gut, which was purple from the long-continued strangulation, carefully returned to the abdominal cavity with the aid of the forefinger and the ivory handle of an exploring needle. The tumour contained the greater part of the small intestine from near the duodenum to the lower end of the ileum, but no omentum was found in the sac. The opposite walls of the canal were now brought together by one silk suture, the ends of which were drawn out of the external wound, which in turn was closed by several stitches, and dressed with oxide of zinc ointment, covered with picked lint. The patient made a rapid recovery, without a single bad symptom. A year has elapsed since the operation, which may be considered as a final success. Until lately a truss has been constantly worn, but the hernia is now radically cured, and the support no longer needed.

17. *Notes of a Case of New Growth in the Navel.* By Dr. Wulkow, of Pirna ('Schmidt's Jahrb.,' h. 2, 1876).—The author cites a case of the simultaneous appearance of a carcinomatous tumour of the stomach and navel, which according to his conjecture were developed from the same cause. He treated a patient, who hurrying along one dark night ran against a pillar, injuring himself in the region of the stomach, since which time he complained of pain in the abdomen, which at first seemed to be stomach catarrh. A year later a humid, irregular, prominent tumour appeared in the navel about the size of a plum, and the skin in its neighbourhood was red and infiltrated. A process of the mass could be made out along the ligament suspensorium hepatis. Shortly afterwards there were symptoms of deep-seated pain, more in the anterior portion of the stomach. The patient was treated with condurango, but soon sank; and the post mortem confirmed the diagnosis of cancer of the stomach. The growth in the navel also showed clearly carcinomatous structure, of similar nature to that of the tumour of the stomach.

(For an essay on operative treatment in cases of carcinoma involving the stomach reference may be made to a paper—"Die partielle Magen resection, eine experimentelle operative Studie," &c.—Gussenbauer und Winiwarter, 'Archiv f. Klin.-Chir.,' B. 9, h. 3, 1876.)

(ED. REF.)

## REPORT ON PHYSIOLOGY.

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## BLOOD.

1. L. MALASSEZ. *Recherches sur quelques variations qui présente la masse totale du sang.* 'Archives de Physiologie,' 1875, p. 261.
2. A. EWALD. *Nachweis von Zucker und Blute eines gesunden Menschen durch Reduction, Gährung und Drehung.* 'Berlin. Klin. Wochenschrift,' 1875. Nos. 51 and 52.
3. ALEX. SCHMIDT. *Ueber die Beziehung der Faserstoffgerinnung zu den Körperlichen Elementen des Blutes.* 'Pflüger's Archiv,' xi, p. 291.
4. OLOF. HAMMARSTEN. *Untersuchungen über die Faserstoffgerinnung.* 'S. A. Nov. Act. Soc. Scient. Upsal,' Sér. iii, B. x, 1875. Abstract in 'Centralblatt,' 1876. p. 249.
5. Dr. A. RÖHRIG. *Ueber die Zusammensetzung und das Schicksal den in das Blut Eingetretenen Nährfette.*
6. A. PUPIER. *Action des Alcalines sur la Composition du Sang,* &c. 'Comptes Rendus,' lxxx, p. 1146.

1. M. MALASSEZ suggests the use of the term "globular capacity" to indicate a quotient obtained by dividing the absolute number of blood-corpuscles by the weight of the animal expressed in grammes. A rabbit weighing 2450 grammes has 919,450 millions of blood-corpuscles, and has hence a "globular capacity" of 373 millions. By the term "globular richness," M. Malassez signifies the number of blood-corpuscles contained in a cubic millimètre. If these two amounts be followed through the animal series it will be found that the globular capacity is greatest in mammals (bat, 630 millions; rabbits, 373 millions); and next in the birds, in which it is somewhat smaller. It is much less in teleostean fishes, and still less in cartilaginous fish (sharks and rays) and the batrachia (torpedo, 2·6 millions; frog, 17 millions; proteus, 2 millions; axolotl, 1·4 millions). The "globular richness" diminishes in the animal kingdom in the same direction as the blood capacity: the two curves, however, are not quite parallel, the "globular capacity" diminishing more rapidly than the "corpuscular richness." This smaller diminution of corpuscular richness has as a result that the greater diminution of the "globular capacity" is to some extent compensated. M. Malassez has made the influence of age on the number of the blood-corpuscles the subject of extended researches in rabbits, rats, guinea-pigs, dogs, cats, the embryos of birds, and the larvæ of frogs. In mammals, as a rule, both the globular capacity

and globular richness rise immediately after birth, and attain their acme in the third or fourth week of life; it then begins to fall, and sinks below its original amount. Both amounts increase again considerably in adults. In fowls the globular capacity scarcely varies during the whole period of brooding; after hatching it sinks very considerably, and in adults it rises again without even attaining its original embryonal amount. Researches on animals in various hygienic conditions gave as a general result that the corpuscular capacity invariably diminishes when these conditions are unfavorable. M. Malassez examined the blood of two persons from one of whom the blood had been drawn which was transfused into another. In the former the quantity of blood present amounted to one seventieth of the weight of the body, in the other to one ninth.

2. Ewald has had the opportunity of testing the blood for sugar in a healthy man, who died suddenly from rupture of the pulmonary artery. The blood obtained from the resulting hæmothorax treated with absolute alcohol, lead acetate, &c., in the usual method gave a watery extract, which reduced copper, underwent fermentation, and rotated the ray of light to the right.

3. Alexander Schmidt, who has strongly maintained the compound nature of fibrin, observes that the artificial formation of fibrin from its two generators (and ferment, which as a rule is adherent to the fibrino-plastic substance) takes place best and most readily when one of the two substances is employed in its natural solution, whilst it occurs feebly and imperfectly or not at all if the two bodies are mingled in solution, in weak solution of soda. From recent experiments he has satisfied himself that the presence of neutral salts is requisite for the formation of fibrin, just as it is for the coagulation of albumen at a high temperature. If from two fluids which when mingled coagulate the salts are removed by dialysis and the precipitated substances (fibrin-forming substances) be dissolved with a minimum amount of soda, no coagulation results from their admixture, but if one of the diffusates be now added to the mixture fibrin separates out. The same effect is produced by the addition of some solution of common salt to the extent of 0·8—1 per cent. It thus appears that a certain relative amount of salt is requisite, and an explanation is afforded of the circumstance that the fluids of the body give, if diluted, less fibrin. Schmidt describes the mode in which the several factors of fibrin may be obtained. For the ferment he recommends that alcohol should be allowed to act on albumen for three or four months, otherwise the solution of the ferment may contain fibrino-plastic substance. 2. Fibrinogenous substances are contained in quantity in the pericardial fluid of the horse and the fluid of hydrocele. 3. Fibrino-plastic substance is best obtained from ovalbumen which at most only contains traces of the ferment. If the three substances are thus separately obtained it is easy to show that their concurrent action is required for the formation of fibrin coagulation, which if they be added together take place either with neutral, feebly alkaline or feebly acid reaction. The quantity of the fibrin obtained depends on the temperature and its physical

condition on the rapidity of its formation; the slower it is the looser.

4. The starting-point of Hammarsten's investigations was the observation that on the addition of calcium-chloride to the fluid of hydrocele which had previously been mingled with fibrin-ferment the process of coagulation was (1) considerably accelerated, and (2) that the quantity of fibrin excreted was considerably augmented, as was shown by weighing the ashes of the fibrin. Hammarsten, therefore, naturally asked himself the question whether the paraglobulin or fibrino-plastic substance present did not act in the same way as the addition of calcium-chloride, and whether consequently the theory of Schmidt, according to which a chemical combination takes place between the fibrinogen and the paraglobulin, could not be dispensed with. To decide this question Hammarsten made a great number of experiments. He first endeavoured to settle the question whether fibrin is really produced by the union of two kinds of albumen, the fibrinogenous and the fibrino-plastic substance. Thirty-one specimens of hydrocele fluid were experimented on, most of which are for the most part, according to Schmidt, "free from paraglobulin." Of these six coagulated spontaneously in the course of twenty-four hours, six more in the course of a few days, and the nineteen others refused to coagulate spontaneously. Of these last, ten coagulated after the addition of a solution of ferment, five after the addition of fibrin and fibrino-plastic substance, and four not at all. The solution of ferment was obtained in accordance with the direction given by Schmidt by extracting the dried coagulum precipitated from blood serum by alcohol with water. The expressions paraglobulin and fibrino-plastic substance are uniformly considered by Hammarsten to be identical. After he had satisfied himself that calcium-chloride might be termed fibrino-plastic with as much propriety as the paraglobulin, he proceeded to inquire whether other substances might not act in the same way. He first experimented with casein, which had been freed from fat and lactin by repeated solution in weak solution of soda, filtration, and reprecipitation by acetic acid. This was suspended in water and added to the hydrocele fluid, but no influence was observed to be exerted on coagulation. On the supposition that the mode of division of the casein might have a certain influence on the coagulation, the following experiment was instituted. Serum of the blood of the horse was diluted with nine volumes of water, and the paraglobulin thrown down in the course of twenty-four hours by the addition of acetic acid. The serum, now free from paraglobulin, was mixed with an alkaline solution of casein, and this again precipitated by the addition of acetic acid. The precipitate thus obtained, which on exposure to air changed into a sticky and even syrupy fluid, was found to be easily soluble in a 1 to 7 per cent. solution of common salt. This casein acted exactly like paraglobulin, hastening the coagulation and increasing the amount of fibrin. It might still be questioned whether the casein remains unchanged in its essential characters. But it is so, the neutral solution in common salt coagulating on the addition



of rennet. On the other hand, it is certainly very probable that, in being obtained by the above-mentioned method, it is rendered impure from the presence of certain constituents of serum. It thus appears, nevertheless, that there are three substances which promote coagulation, and it is of importance to determine what it is that is common to all. With this end in view it was necessary to enter more minutely into the conditions of coagulation and to work as far as possible with pure materials, pure fibrinogen, ferment, and paraglobulin. Hammarsten adopted the following method to obtain the fibrinogen. The blood of the horse was received into vessels containing one fifth of their volume of a concentrated solution of sulphate of magnesia, so that a mixture of one volume of saline solution and four volumes of blood resulted; after standing several days the mixture was filtered. The clear, sometimes reddish filtrate was mingled with an equal volume of concentrated solution of common salt; when the fibrinogen was precipitated, it was further purified. It was used in the form of a solution in water containing about one per cent. of common salt. This solution did not coagulate spontaneously, but did so on the addition of fibrine-ferment. It was obviously of great importance to show that the fibrinogen solution contained no paraglobulin, and yet that notwithstanding the absence of this it coagulated. This proof may be shown by the addition of some common salt to the fluid; complete precipitation occurs and no albumen can be demonstrated, which can always be done when paraglobulin is present, as is shown by special experiments. The second part of Hammarsten's paper deals with the explanation of the influence that addition of paraglobulin exerts upon the rapidity of the coagulation and the quantity of fibrin. A. Schmidt noticed that less fibrin is obtained from strongly than from weakly alkaline fluid, and that its quantity can be raised by neutralisation; nevertheless, all the fibrin is not obtained under these conditions, since a part is again retained in solution by the salt resulting from neutralisation, and with a certain amount of alkali present the addition of alkali is without effect, because all the fibrin produced remains dissolved.

Many hydrocele fluids, containing a very small amount of fibrinogen, give no coagulum (after the addition of a ferment), though they do so if they have been previously neutralised. The influence of alkalescence must necessarily be greater in fluids poor in fibrin than in those rich in fibrin, since the quantity of the fibrin held in solution by the alkali is an absolute amount, so that the increase of fibrin caused by neutralisation in fluids rich in fibrin is relatively less considerable than in those poor in fibrin. Neither the alkalies nor the salts act upon the ferment or upon the fibrinogen, since by mere change of the conditions, as, for example, the act of neutralising, causes the separation of fibrin from a previously non-coagulating fluid; they hold the fibrin in solution. Fibrin, when once separated from pure neutral solutions and well washed, is insoluble in salts and alkalies, and that separated from alkaline solutions has a gelatinous appearance, and is again after long standing redissolved in the solu-

tion from which it is separated. In one experiment neutral solution of fibrinogen was taken; ferment was added to it to one half (A); a little very dilute soda solution was added; the other (B) was left without addition; B coagulated in thirty minutes; A after the lapse of three or four hours. After thirty-one hours the fibrin in B remained undissolved, whilst it had perfectly redissolved in A. The fibrin of B when washed did not dissolve in alkali. The fibrin thus comes to resemble paraglobulin, and can induce coagulation in a solution of fibrinogen. It can be obtained like paraglobulin by greatly diluting the fluid A, and transmission of  $\text{CO}_2$ , and it has the same relations with salts. In sufficiently strong saline solutions no separation of fibrin occurs, yet the fibrin is formed and remains dissolved in the fluid. If now an equal volume of concentrated NaCl solution be added, or if the fluid be sufficiently diluted with water, the fibrin separates. This modification of fibrin may be called "soluble fibrin." On the ground of these observations Hammarsten seeks to explain the rôle of calcium-chloride, of paraglobulin, and of soluble casein. The action of calcium-chloride probably depends on the fact that it undergoes decomposition with the free (carbonated) alkali, calcium carbonate and potassium chloride being formed. Its action is, therefore, analogous to that of acids. The action of paraglobulin is still more analogous to that of acids. The paraglobulin, according to the author, combines with or invests the alkali and salts, thus setting aside the disturbing influence which these would exert on the act of coagulation; it acts also to a certain extent through the ferment that cleaves to it; casein rendered impure by the presence of the constituents of serum, acts like paraglobulin. Hammarsten is of opinion that this impurity is really lecithin; and this is the reason why vitellin greatly hastens the act of coagulum.

5. Dr. A. Röhrig gives the results of a series of experiments he has made to determine the mode in which fatty substances enter and are decomposed in the blood. It is commonly stated that the blood contains fatty acids in combination with alkalies, in fact, soaps; but he adduces various arguments to show that soaps cannot enter into the blood, and comes to the conclusion that the fats enter the blood as such, and through the thoracic duct alone. When a dog is fed with large quantities of fat, fat is found both in the serum and in the clot of blood. He thinks the mode in which the fats may be disposed of may be twofold—either they pass unaltered out of the blood, or they undergo decomposition in the blood and the products are eliminated.

6. M. Pupier, from observations made upon a case under his care, arrives at the conclusion that the employment of alkalies is not so prejudicial to the constitution of the blood as is generally supposed. The patient was forty-seven years of age, and for nineteen years had daily taken from 240 to 280 grains of bicarbonate of soda, and has become plethoric rather than anæmic. The number of blood-corpuscles estimated by Malassez's method amounted to 5,406,000 in one cubic millimètre, the normal number being 4,500,000. A dog

to which 1300 grains of bicarbonate of soda was administered in the course of a month, in the form of Vichy water, did not diminish in weight, but the number of its blood-corpuscles rose from 4,239,000 to 5,910,000, and fell again after disuse of the soda in twenty days to 4,480,000. The same results were obtained in other experiments.

#### CIRCULATION—RESPIRATION.

1. NAWROCKI. *Ueber den Einfluss des Blutdrucks auf die Häufigkeit der Herzschläge.* 'Beiträge z. Anat. u. Physiol.' Festgabe. f. C. Ludwig. p. 205.
2. *Recherches Experimentales sur la Respiration Pulmonaire chez les grands Mammifères domestiques.* Par M. ANDRÉ SANSON (de Grignon). 'Robin's Journal de l'Anatomie,' Mai et Juin, 1876.

THE results of Nawrocki's experiments are, that if all the nerves distributed to the heart be divided, the frequency of the pulse is rendered entirely independent of changes in the blood pressure, which is essentially in accordance with the statement of Knoll and Worm Müller; on the other hand, the force of the individual acts of systole increases with increasing blood pressure, and *vice versa* diminishes with lower pressure, when the vagi are preserved intact. Nawrocki, like Bernstein, Asp, and Knoll, found that the frequency diminished as soon as the pressure was augmented, and increased with diminished pressure. In all these experiments the preservation or section of the accelerating nerves was of no importance, in regard to the frequency. In order to eliminate statistically the irregularities so frequently observed in these experiments. Nawrocki experimented on 400 animals, dogs, cats, and rabbits; these experiments are divisible into three sets: 1. Those made with hearts completely detached from all nervous influence, the spinal cord, the vagi, the depressors, and cervical sympathetic being all divided; the division of this last nerve is, according to Nawrocki, superfluous, since the cervical sympathetic properly contains no accelerating or otherwise regulatory nerves of the heart. 2. Those made with preservation of the accelerating nerves, the cervical portion of the spinal cord and dorsal portion of the sympathetic being intact, the vagi and spinal cord in the dorsal region remaining intact in one set of experiments and being divided in another. 3. With intact vagi, partly in animals otherwise undamaged, and partly after division of the accelerating nerves (section of the spinal cord). The elevation of the blood pressure was induced by compression of the aorta without opening the abdominal cavity by irritation of sensory nerves, by transfusion of the defibrinated blood of the same species, and, lastly, in some experiments in which the spinal cord was divided in the dorsal region by electrical stimulation of the distal surface of the medulla. The diminution of the pressure was affected by irritation of the depressor section of the splanchnics and by bleeding.

At the conclusion of an elaborate memoir on the pulmonary

respiration of the larger domestic mammals, M. Sanson gives the following résumé:—1. The intensity of the respiratory function varies with the species of animal. The weights being equal, the equinæ eliminate more carbonic acid than the ruminants in the same space of time. 2. In each species the races, and in each race the variety having the least weight, have the most active respiration. These present relatively the largest pulmonary surface. Amongst horses those of English breed exceed all others in the spaciousness of their thoracic cavity and in the volume of their lungs. Their lungs also present smaller alveoli. Amongst the bovidæ the observations of Baudement, which have been confirmed by all other observers, show that the weight of the lungs diminishes relatively to the weight of the body, and the capacity of the thoracic cavity diminishes also in proportion as the race or variety is more premature or precocious in the ossification of their skeletons. 3. Sex influences respiration, being more active in the male; and it is well known that his pulmonary capacity is the largest. 4. The young eliminate more carbonic acid gas than the old. The respiratory rhythm and the number of respirations are relatively less in the old. 5. Food, providing it be sufficient to maintain the animal in its normal state, has no influence on the amount of carbonic acid gas eliminated, whatever may be its quantity or quality. 6. Respiratory effort, which causing increased elimination of carbonic acid gas whilst it is being made, has no subsequent action. Animals used for draught do not during repose give off more of this gas than animals that do no work. 7. Temperature has a direct and important influence on the elimination of carbonic acid gas. The quantity eliminated is in direct proportion to the height of the temperature. Contrary to what is generally stated, the amount of carbonic acid gas eliminated by the respiration diminishes as the temperature falls. 7. The barometric pressure acts inversely to the temperature. The elimination diminishes in proportion as the pressure rises and augments as it falls. The influence of temperature and pressure consequently compensate one another within certain limits.

#### SECRETION—PANCREAS—LIVER—INTESTINE—SKIN.

1. R. HEIDENHAIM. *Beiträge zur Kenntniss des Pankreas.* 'Pflüger's Archiv,' B. x, p. 557.
2. E. FLEISCHL. *Von der Lymphe und den Lymphgefässen der Leber.* In 'Ludwig's Arbeiten,' 1875, p. 24.
3. Dr. M. ABELES im Carlsbad. *Verbreitung des Glycogens im Thierischen Organismus.* 'Vorläufige Mittheilung.'
4. JNO. MUNK. *Ueber die Harnstoffbildung in der Leber, &c.* 'Pflüger's Archiv,' B. xi, p. 41.
5. M. MARKWALD. *Ueber Verdauung und Resorption im Dickdarm des Menschen.* 'Virchow's Archiv,' lxi, p. 505.
6. E. KLEIN. *Observations on the Structure of the Spleen.* In 'Quart. Journ. of Microscop. Science,' Oct., 1875.

7. Dr. DROSDOFF und Dr. BOTSCHETSCHKAROFF. *Die Milz Contraction und ihre Beziehung zur Leber während Milz Nerven-Reizung.* In 'Centralblatt f. d. Med. Wiss.,' 1876. No. 5.
8. E. HÖRSCHELMANN. *Anatomische Untersuchungen über die Schweissdrüsen des Menschen.* Inaug. Diss. Dorpat, 1875.

1. Heidenhaim describes the changes observed in the pancreas according to the stage of digestion. 1. During fasting; the granular internal zone occupies the larger, the homogeneous internal zone the smaller part of the cells. 2. During the first period of digestion, when secretion takes place with the greatest activity, contraction of all the cells takes place consequent on the consumption or using up of the granular internal zone, notwithstanding some addition of new material is made to the external zone. 3. During the second period of digestion, when secretion diminishes and gradually stops, there is new formation of the granular zone at the cost of the homogeneous outer zone, which gradually becomes extremely thin; the whole cell becomes greatly enlarged. 4. After long fasting the cells still retain a large size. During the period of physiological activity there is a constant change in the cells—consumption within, addition without; internal conversion of the granules into the constituents of the secretion; external application of nutritious material to the formation of homogeneous substance, which is again converted into granular material. During the intervals of digestion he thinks the cells store up, not pancreatine, but another substance, which he proposes to call zymogen and which he believes to be formed by a combination of pancreatine with an albuminate. As the gland goes on secreting the amount of zymogen, the gland diminishes and regenerates again as the gland passes into a state of rest. The proportions of water and of pancreatin vary and are under independent nervous influence.

2. Fleischl observes that if the ductus choledochus be tied in a living dog and after a short space of time the lymphatics are exposed which proceed from the porta hepatis to the cisterna chyli, the fluid they contain is found to have assumed a yellow tint and to contain bile, for on the addition of nitric acid the presence of the biliary colouring matter is clearly shown (Gmelin's reaction), and by the usual tests the presence of the biliary acids in the fluid of the thoracic duct can be demonstrated. Further experiments have led him to the following conclusion:—That if the natural paths of exit of the bile be occluded it enters the lymphatics of the liver and passing from thence into the thoracic duct it gains entrance into the blood. But if in addition to occlusion of the ductus communis choledochus the thoracic duct be also tied, the bile does not gain any entrance into the blood, or at most only faint traces of it can be found in the blood. In order to determine the path by which, when the ductus communis was tied, the bile reached the blood-vessels, Fleischl made very careful sections of the injected organ and describes minutely the connective tissue surrounding the ultimate ramuscles of the hepatic vein. He believes he has been able to make out the exist-

ence of definite walls to the finest branches of the biliary capillaries which have hitherto been regarded as only grooves on the adjacent flat surfaces of the bile-cells, but he has not been able to discover any direct communication between the connective-tissue-corpuscles and their prolongations and the biliary tubes.

3. Dr. Abeles states that glycogen has been rarely found elsewhere than in the liver and the transversely striated muscles. He has, however, satisfied himself of its presence in the healthy spleen, lungs, and kidneys of dogs, which had been previously fed for three days on bread.

4. M. Munk has made a series of comparative researches on the proportion of urea contained in the blood and liver of the same animal. The urea was first separated from the blood or liver by precipitation with Liebig's solution. This precipitate was treated with hydrogen sulphide, and the actual amount of urea ascertained by Bunsen's method by heating with barium, chloride, and ammonia. The liver was rubbed down with alcohol, but otherwise treated in the same way. Some of the results were as follows:—Dog No. 1. Blood, 0·053 per cent.; liver, 0·039 per cent. Dog No. 2. Blood, 0·052; liver, 0·046. Dog No. 4. Blood, 0·041; liver, 0·030. The proportion of urea was thus always smaller in the liver than in the blood, and Munk therefore thinks there is no ground for attributing to the liver the power of forming urea.

5. M. Markwald's experiments were made on a case of artificial anus consequent on hernia, the part of the intestine opened being the junction of the sacrum and colon; the two portions of intestine were entirely separated from each other, and the mucous membrane of the large intestine was accessible to observation throughout its whole extent; the temperature of the large intestine was 37·6° C.; the peristaltic action was very energetic. The patient was forty-nine years of age, rather delicate make, but of good general health. The first series of experiments related to the sugar-forming ferment of the large intestine. Sponges were introduced to some distance into the large intestine, and were allowed to remain for two hours, then withdrawn and pressed, when a tenacious nearly clear fluid of strong alkaline reaction, and containing a trace of albumen, was obtained; it had no action at 40° on starch paste. Starch paste enclosed in muslin bags, and introduced into the intestine, exhibited no trace of the formation of sugar even after the lapse of from four to six hours.

A second series of researches related to the digestive powers of the large intestine, fibrine partly free and partly enclosed in bottles was introduced into the large intestine, and one remained for no less than twenty days. The quantity of the fibrine considerably diminished, the products of its metamorphosis being peptones, tyrosin, and indol. The mass was filled with bacteria. Markwald regards it as a proces of putrefaction. In twenty-six hours 84 per cent. of a mass weighing about seventy grains was dissolved; the coagulated white of egg also diminished considerably in weight, the diminution, however, not being proportionate to the time. The

amount dissolved was about 60 per cent. in forty-eight hours; the products were the same as in the case of fibrin. If large quantities were introduced, half a pound or more, the diminution in weight was much less, not amounting to more than 30 per cent. The occurrence of absorption by examination of the amount of nitrogen in the urine did not give any satisfactory results, though some increase was observed. A third series of observations were made on the absorptive power of the intestine. These showed that water could be absorbed by the large intestine, though very slowly, twelve hours being required for the absorption of 250 ccm. An increase in the amount of nitrogen in the urine, when peptone prepared from fibrin was introduced, showed that some absorption of this material took place; if introduced in large quantities the peptones appeared to irritate the intestine and to produce diarrhoea. Fluid albumen was not absorbed. That the function of the large intestine can be dispensed with, without disturbance of the general health, is shown by the circumstance that the patient remained in thoroughly good health after the fistula had existed for two and a half years.

6. Klein finds that in the dog the capsule contains in that half which is next the parenchyma a continuous mass of unstriated muscle, the fasciculi of which are arranged parallel to the longitudinal axes of the whole organ. Above this longitudinal coat is a more sparing layer of similar muscular fibres running at right angles to the former and imbedded in connective and elastic tissue. The trabeculae are almost entirely composed of muscular fibres. In the spleen of monkeys the capsule contains a thin layer of longitudinal muscles in the deeper part only, whereas the trabeculae possess a great amount of muscular tissue. In the spleen of man the capsule contains sparingly muscular fibres, and generally at those points only where the trabeculae are inserted into the capsule. The larger trabeculae, including the larger branches of the splenic artery and vein, contain in their periphery numerous bundles of unstriated muscles; the smaller trabeculae also contain muscular bundles. In the pulp he finds not a reticulum of fine fibres as generally admitted, but a honeycomb of membranes, including spaces of various sizes containing a few blood-corpuscles and numerous nuclei, which are either large, pale, and round or smaller, elliptical, spherical, or irregular-shaped. The structure of the pulp, as he observes and describes it, is, he thinks, consistent only with the theory that the venous radicles of the pulp are represented by a labyrinth of spaces in the matrix as advocated by W. Müller, Frey, and others. The lymphoid corpuscles which are produced in such numbers in the spleen appear to be developed from the general cellular stroma of the pulp by a process of budding. In regard to the relation of the matrix of the pulp to that of the adenoid tissue of the arterial sheaths and to the Malpighian corpuscles, he thinks the one passes gradually into the other. He finds numerous lumps of yellowish pigment in the pulp of the human spleen which he believes to stand in intimate relation to coloured blood-corpuscles that have been taken up by lymphoid cells and broken up into small particles within their structure. The

multinuclear cells of Kölliker have a real existence and originate in the spleen pulp.

7. Drs. Drosdoff and Botshetschkaroff state that they were led to undertake their researches in consequence of the observations of Prof. Botkin, that the liver augmented in volume in patients in whom the spleen had been made to contract by the passage of induced currents of electricity. They laid open the abdomen of a narcotised dog in such a way as to expose both the liver and the spleen. They then carefully enveloped the spleen in the mesentery, and partially withdrew it, keeping the surface of the membrane constantly wet with warm water. The splenic arteries with their accompanying nerves were easily exposed, and the latter were isolated and divided. The spleen immediately began to enlarge. Irritation of the peripheric extremities of these nerves caused the spleen to contract; during this procedure the liver was closely watched without being removed from its bed. A manometer was introduced into the splenic vein, in order to indicate changes of pressure in the foetal system. The experiments led to the following results:—

1. Section of the nerves forming the plexus lienalis causes the spleen to augment in all its diameters to the extent of some centimeters, and irritation of the distal cut extremities causes it to diminish. As soon as the latter process commences the liver begins to enlarge, the tubules becoming more sharply contoured, their colour a more pronounced red, the borders of the organ more rounded, and the whole texture firmer. 2. If whilst the spleen is swollen the liver be pricked by a needle, scarcely any blood flows; but as soon as the spleen has been caused to contract by the passage of the electric current, it flows so freely as to empty the liver. 3. After each contraction of the spleen the number of white corpuscles in the liver increases, as the authors of the paper have ascertained by direct enumeration. 4. On irritation of the splenic nerves the pressure in the splenic vein rises. 5. As soon as the irritation is no longer applied the pressure falls to its original level, though before the spleen attains its original volume, indicating that the effect observed is not altogether a vaso-motor action, but is due to the co-operation of other muscular elements, which have indeed been demonstrated in the lesion of the spleen. 6. Ligature of the splenic vessels materially diminishes the manifestation of the above effects.

8. According to Hörschelmann, sweat-glands occur in all parts of the body, even on the concave side of the concha of the ear, where their presence has been hitherto overlooked. The gland-ducts or tubes never divide dichotomously in the large glands of the axilla. The diameter of the excretory duct is always smaller than that of the tube. The stratum Malpighii dips down in the form of a conical process between two papillæ in the cutis towards the excretory duct; when the two are in contact, the excretory duct begins its corkscrew-like windings. Hörschelmann distinguishes small and large sweat-glands. In the latter the tube varies in diameter, in the latter it remains the same. Muscles are found in all sweat-glands, excepting those of the scalp. They always lie close beneath the epithelium.



The epithelial cells are polyhedric, and their basal extremities are often dentated. They sometimes possess a cuticula. The epithelium in the excretory ducts consists in the small glands always of at least two cell layers, which increase in thickness towards the exterior. The innermost cell layers always possess a cuticula.

#### NERVES.

1. R. ARNDT. *Untersuchungen über die Ganglien-Körper der Spinal Ganglien.* 'Archiv f. Microscop. Anat.,' Band xi, p. 140.
2. TH. TREITEL. *Eine neue Reaction der Markhaltigen Nervenfasern.* In 'Centralblatt f. d. Med. Wiss.,' 1876. No. 9.
3. W. KRAUSE. *Die Nerven-Endigung in der Retina.* 'Archiv f. Microscop. Anatomie,' Band xii, heft iv, p. 742.

1. R. Arndt observes that the fundamental form of the cells of the spinal ganglia is that of an irregular more or less flattened disk. They are at least bipolar, but he believes that multipolar ganglion-cells also exist, of such form that in addition to two strong and easily perceptible processes they send forth a number of finer processes which, however, may easily be torn or overlooked. He has *not* been able to satisfy himself of the existence of unipolar ganglion cells, though like other observers he finds many examples in every preparation. The apolar cells which he has also discovered in the spinal ganglia he regards as the result of an anomalous process of development. The two chief processes of any ganglion-cell usually arise in immediate proximity with one another. In many cases each process is enclosed in a special sheath given off from the capsule of the cell; whilst in others the two processes are enclosed in a common sheath. They are almost always medullated even at their origin, though non-medullated specimens are sometimes met with.

2. M. Treitel states that in the course of some investigations he has recently made on the anatomy and pathology of the eye he noticed a remarkable peculiarity of some anilin dyes, namely, that fuchsin, anilin blue, and the iodine violet recommended by Jürgens, tint the medullated nerve substance of healthy nerve-fibres very intensely, whilst they act on degenerated nerve-fibres much less strongly, and do not colour connective tissue at all or but very slightly. He thinks that although these substances may not supplant the use of gold chloride, they may yet prove very serviceable in rapidly bringing the medullated nerve-fibres into view, a few minutes being all the time required. These dyes have the advantage over gold chloride of staining preparations that have long been kept in Müller's fluid. The precise method he adopts is as follows:—A fine section of the optic nerve is placed for about a minute in a very dilute solution of the iodine. The preparation is then carefully washed in distilled water and mounted in glycerine. The normal nerve fasciculi then appear tinted of a violet colour, whilst the connective tissue fasciculi appear by contrast of a bright yellow; any degenerated

fibres are much more feebly stained. The walls of the vessels are of a clear violet, and can be easily recognised. The preparations cannot be put up in resin, as the preliminary treatment with alcohol and turpentine discolours them.

8. W. Krause advances the following reasons *against* the hypothesis of an anatomical continuity of the fibres of the optic nerve with the epithelial cells of the retina, *i.e.*, with the rods and cones. (1.) The rods and cones are connected with the connective-tissue-cells of the membrane fenestrata, and the radially running supporting fibres which are undoubtedly composed of connective tissue. (2.) The parallax of the "vascular figure" is explicable on the view that light is perceived in the plane of the choroid. (3.) The results of sections of the optic nerves and the conditions present in anencephalic fetuses. (4.) The outer segments are excluded in birds and amphibia by the presence of coloured oil drops. (5.) In the outer segment the occurrence of lamination in the inner segment, the presence of fat drops, and of ellipsoidal lenticular paraboloidal and hyperboloidal bodies in many animals in the rod and cone granules, the occurrence of transverse striation, all constitute so many arrangements that are undoubtedly of a dioptric nature and are therefore of importance. (6.) The epithelial cells of the retina proceed from the ectoderm (epiblast). In the solitary instance where the termination of the nerve fibrils in homologous cell-layers has been ascertained with certainty, it takes place *between* the cells (anterior epithelium of the cornea, Cohnheim.)

## REPORT ON TOXICOLOGY, FORENSIC MEDICINE, AND HYGIENE.

By BENJAMIN W. RICHARDSON, M.D., F.R.S.

### I. TOXICOLOGY.

*A Case of Tobacco Poisoning.*—Dr. John W. Bigelow, of Albany, New York, relates that on the 28th of August, 1875, he was hastily summoned to attend Mr. T. S—, a clerk, aged twenty-six years, who had been suddenly seized with convulsions while walking with a friend in the street. He was stricken without premonition, and was conveyed to a neighbouring store, where Dr. Bigelow found him. His skin was pallid and anæmic; his features pinched and contorted; his pulse irregular, variable, and intermittent—at one time 136 in the minute, and in a short time down to 38. The heart beat very irregularly, as evinced by palpitation and tremulous motion of the organ, especially when the man was lying upon the left side. The heart sounds were muffled, and seemed almost to run into each other. The temperature ranged from 98° to 99·5° Fahr. The lips were bloodless, the eyes staring, the pupils dilated. He com-

plained of great pain and distress in the left side of the chest, especially around the præcordial region; suffered from dyspnœa, drew long sighs, made a gulping effort at emesis, had hiccough and cold perspiration, and presented great nervous prostration.

These symptoms were rapidly succeeded by clonic convulsions, which produced great muscular agitation, particularly of the extremities. The teeth were knit together, hands tightly clenched, the legs flexed and extended in rapid alternation. With the cessation of these symptoms the patient complained of pain more or less complete, especially of the left side of the extremities, and of the tip of the tongue; he also spoke of excessive languor and of nervous tremor. After a longer time Dr. Bigelow observed a cataleptic condition of the arms and legs. If a leg or arm were extended or flexed or uplifted it would retain that position for at least five minutes, and the patient, although conscious, was yet unable to help himself or to control the excessive muscular perturbations. This condition passed off, and was followed by hysteric tremors with convulsive twitching of the flexor muscles of the whole body, accompanied by an agonised apprehension of some rapidly approaching catastrophe, the result of which would be death. This fear was in some subsequent attacks the cause of prolonged mental and bodily excitement. Conversation, rapid walking, or any violent motion of the attendants would provoke this spasmodic attack and produce great nervous excitability. He was treated by the hypodermic injection of morphia and with bromide of potassium and ammonia. He was removed home and the urgent symptoms subsided.

On inquiry, Dr. Bigelow found that this was the third attack of a similar kind to which the patient had been subjected. The patient had smoked tobacco since he was twelve years of age, some days using as many as twelve cigars, and often substituting for luncheon three or four strong ones. He had little or no appetite for most of the time, was pale and cadaverous, languid and weak; ate but little and at irregular intervals; but from the time he rose in the morning to his retirement at night he was never without his "weed." He was restless, would start up and jump in his sleep, and had become irritable in disposition and enfeebled. A searching scrutiny of his family history failed to elicit any trace of epilepsy or other nervous disorder, and with regard to himself he denied any other sickness than the present.

Under quinine, iron, and strychnine, the health of this patient improved, but on September the 5th Dr. Bigelow was again summoned to attend him, as the messenger said, for the "worst fit he had ever had." On arriving Dr. Bigelow found the patient reclining on a couch, presenting the appearance, in speech and looks, of an acute maniac. He was seizing his friends and imploring them to save him from some indescribable "something," which was stifling him. The next moment he fell into the most violent convulsions. He presented the same symptoms in rapid succession as have already been narrated, excepting that his fear of death had become a mania. On inquiry it was found that he had become fretful for a day or two,

neglecting his meals, and smoking from eight to fourteen cigars daily. He was again put under treatment, and on the 6th of September was better, but still suffered severely from hysteric and ataxic tremors of the limbs; he also complained of great distress in the cardiac region, which would occasionally precipitate a transient convulsive action of the whole body. He could hardly sit upright, and called attention to great numbness of the extremities and tongue. He felt cold and chilly. On the 30th Dr. Hun was called in, and confirmed the diagnosis that had been made by Dr. Bigelow, that the cause of the symptoms was the poison of tobacco. On the 11th the patient went into the country, and returned on the 30th fully restored to health, and better than he had been for several years. Since then, on three occasions, when he has smoked from four to six cigars in succession, the symptoms have returned. He has at last given up smoking entirely, and now enjoys the best of health.—*New York Medical Journal*, April, 1876.

*Poisoning by Cyanide of Potassium.*—Dr. Julius A. Post, of Rochester, New York, records that he was called to attend Mr. R—, aged 58 years, who was labouring under symptoms of congestion of the brain, and who died in about half an hour from the time when he, Dr. Post, reached him. The patient followed the occupation of a jeweller, and was in the habit of using cyanide of potassium with gold as an alloy. The vapour of the cyanide, he said, always produced in him the symptoms of brain congestion, and Dr. Post infers that the congestion which ended fatally was, directly, induced by the action of the cyanide vapour. The post-mortem examination, conducted on the day after the death, showed the dura mater and arachnoid to be greatly congested. The pia mater was to all appearance natural, except in an oval spot about an inch long by three quarters of an inch wide, situated about one inch and a half above the internal occipital protuberance, and one inch to the left. The internal surface of this spot, throughout its whole extent, was firmly connected with the surface of the brain, presenting the appearance of old inflammatory action. It did not penetrate the brain-substance, neither was it connected externally with the arachnoid. The arachnoid and dura mater immediately covering it did not present evidence of any inflammatory action. This connecting substance presented the appearance of fibrous adhesions, and when dissected was found to be tough and to cut like cartilage. While dissecting this connecting link the knife struck a spicula of bone nearly half an inch in length, and about the size of a medium pin-point. This particle of bone was firmly pressed upon the brain, but did not penetrate the brain-substance. There was no serum or blood in the ventricles. The cerebrum was slightly congested, but the substance presented nothing abnormal. The cerebellum was very much congested, but quite soft. The organs of the chest and abdomen were healthy.—*Ibid.*

[The case reported by Dr. Post may fairly be classed with others we have reported, in which death has been produced by the inhalation of the vapour of potassium cyanide. The symptoms were such as

would be produced by the cyanide, and the appearances of congestion were such as would follow the effects of the substance as a poison. At the same time the other pathological conditions must be allowed their full influence. They were long present (except perhaps the softening of the brain-substance, which might be due to the frequent action of the cyanide), and they prepared the way for the more rapid effective action of the poison.

While on this subject we may add with advantage some observations on the entrance of the cyanide of potassium into the body by the skin. This danger affects specially those who are engaged in photography. It seems to arise from direct absorption of the poison by the skin, but only when the skin is wounded, abraded, or chapped. Our attention was first called to this subject by a photographer, who consulted us for a series of symptoms with which we were not familiar, and which could not be accounted for by any evidence leading to the suspicion of organic nervous disease. The symptoms came on only when he was at his work, but they lasted for some hours after he had left his work. We suspected they might be due to the inhaling of the vapours which are present in the working room of the photographer; but this theory was excluded by the fact that he had worked many years in the same place without being affected in the same way, and that none of the workmen who were with him in the same room were similarly affected.

These circumstances led us to look out for local absorption, and we found on inquiry that the hands of the man were severely chapped, and that they were so on every occasion when the phenomena recurred; for the phenomena were repeated many times before their cause was discovered. We were now on the right track, and by his giving up that part of the employment which involved exposure of his chapped hands to the solution, the patient experienced a quick cessation of his symptoms, and recovery from them without their recurrence.

The symptoms are exceedingly characteristic; they begin with vertigo. A sense of giddiness is gradually developed, with a sensation as if all objects were passing in a circle, and then as if the body of the affected himself were turning round. At times there is a further sensation of falling, as though, of necessity, the body must pitch forward, and as if the lower limbs were unable to support the weight of the body. These symptoms may last for some hours, and if they are not exceedingly severe they will subside when the work of the day is over, and will not recur until the resumption of labour on the following day. They may be entirely misunderstood, and indeed often are misunderstood. They are attributed to biliousness, or to indigestion, in their lighter manifestations. When they become more severe other symptoms are added, the giddiness or vertigo is attended with nausea and faintness, so that it is impossible to go on with the work. But even from this more extreme condition recovery is rapid after exposure to the cause ceases.

Under still further exposure the body becomes cold, and an extreme shivering takes place, which is succeeded by a prostration that

altogether incapacitates from work, and is connected with a series of new nervous phenomena of great moment. The first of these nervous signs is double vision; the patient, that is to say, in looking at a single object sees as if it were two objects, or as if both eyes were separately discerning the one thing. Finally, there are muscular tremors which are altogether beyond the control of the will. The tremors do not amount to spasms of the muscles, but they are sufficiently active to cause involuntary movements of the limbs, and they are attended with occasional starts and twitchings. The temperature of the body is lowered, and the appetite is greatly reduced: the secretions are confined, the face pale, the action of the heart quick, weak, and irregular, and the sense of exhaustion urgent. The mind throughout is unaffected, but there is perhaps an unnatural tendency to sleep.

The poison being soluble finds its way out of the system with moderate rapidity, and thereupon all the severer symptoms are removed, but some remain for several weeks. The strength returns but slowly; dyspepsia continues as a very troublesome symptom; anæmia is a marked condition, and the blood, which has been rendered very fluid, escapes too freely from wounded surfaces.

The changes which the salt undergoes in the presence of the animal secretions, and in its course through the body, have not as yet been discovered. We have found, however, that the iodide of potassium salt is decomposed within the body, and the probabilities are strong that the cyanide is also decomposed, and that the symptoms are due either to cyanogen, or, as Davanne supposes, to hydrocyanic acid.

While on this topic we may with advantage offer a word of precaution respecting a practice which is commonly adopted by gentlemen who are engaged in photographic pursuits, and which had better be avoided. It is customary for the photographer, when his hands are deeply stained with nitrate of silver, to clean them of the stain by using cyanide of potassium. The process effectually removes the stain, but it is always attended with some risk, and when there are any sores or wounds on the hand, the risk becomes actually dangerous. It is best, therefore, to use some other potassium salt than the cyanide for the purpose named.—B. W. R.]

*Poisoning by Wild Lettuce.*—M. A. Boe records that on the 15th of March of the present year a family consisting of four persons, a child of ten years, her mother, aged twenty-nine years, an uncle, aged fifty years, and a workman of the house, aged twenty-five years, ate at their supper a salad of the season, which had been gathered in the fields situated in the valley of the Garonne. The salad was composed of dandelion, chicory, and also of wild lettuce (*Lactuca virosa*), which was unknown to them, and of which the presence was established next day by parts of the plant that had been thrown aside. All the four ate of this salad; but the uncle, as he did not find the leaves of the lettuce to his taste, drew them from his plate, and partook only of the other parts. The repast took place at seven o'clock in the evening, and they all went to bed at nine. The uncle,

according to his custom, passed a very good night. The workman, a strong and robust man, felt sharp colic towards eleven o'clock at night, and soon had nausea and vomiting, which persisted through the night. At five o'clock in the morning he found repose, but he was surprised when daylight broke to discern that, although the pain had completely ceased, he could not distinguish objects. He took a journal, but could not read any letter, neither large nor small. The mother, a woman of lymphatic temperament, suffered from colic all the night, but did not vomit until eight o'clock in the morning, twelve hours after the repast; then the salad, and the other food which had been taken with it, was ejected. Like the workman, she, too, was surprised to find when the day came that she could neither distinguish objects nor read. The child, a very intelligent and robust boy, who had not been able to sleep according to his custom, was seized towards midnight with a wild merry delirium, which, towards the morning, increased. He sang and jumped on his bed. He did not complain of pain in the abdomen, and had no vomiting.

Called to see these invalids at seven in the morning. M. Boe was struck by the peculiar and identical physiognomy of them. The pupils were very largely dilated; above all in the child, in whom the iris seemed like an exceedingly thin line. None of them could distinguish any object. He pressed the child to fix his attention for a long time on the large letters of the 'Moniteur Universel.' The child mistook the letter "M" in Moniteur for a P, and the "O" for a V. The effort that the child made to arrive at this result brought on an active congestion of the conjunctiva. In his delirium, which continued, he had hallucination of vision. He believed he saw on his bed an inkstand, a soldier, and other objects. The workman having already vomited much, and the mother having vomited again, M. Boe made them take simply at each attack a strong infusion of coffee. For the child he prescribed a dose of emetic tartar, which brought on abundant vomiting, but the nervous symptoms persisted as intensely. All three had several liquid evacuations. At mid-day the invalids were in the same state, and all three were made to take solution of the hydrioduret of potassium. At five o'clock the workman distinguished the large letters, but could not go to his ordinary work of clock-making. The mother saw objects a little better, but could not read. The child had the same lively delirium, which persisted until four o'clock on the following morning, at which hour he went to sleep. The next day, at eight in the morning, the three invalids had recovered; the child only complained of pain in the head. In all, the pupils were still a little dilated, but perfect sight had returned. There was no fever or other morbid symptom. The case is of much interest as showing that the view expressed by Orfila, that the green leaves of the *lactuca virosa* are not poisonous, is erroneous.—*Bulletin Général de Thérapeutique*, April, 1876.

## II. FORENSIC MEDICINE.

*Lateral Hermaphroditism.*—Dr. Paul F. Mundé publishes a case of presumptive true lateral hermaphroditism. The case is not altogether unknown in this country, but has created less attention in professional circles here than it has in any other country. It is remarkable in that it presents for the first time in the history of medicine an example of true presumptive hermaphroditism, authenticated by the direct evidence of many and distinguished observers.

The person who is described by Dr. Mundé was born in Mellrichstadt, province of Lower Franconia, in Bavaria, in the year 1824. The belief being that the infant was of female sex she was named Catherine, her surname being Hohmann. She spent the first forty years of her life as a female, dressed in the clothing and occupied in the vocations of the supposed sex. After her twelfth year she became developed, as it seemed, as a woman; the breasts became large, the organs of generation began to develop, and after puberty sexual propensities directed entirely towards the male sex gradually developed themselves, which propensities in her seventeenth year she began, says the author, to gratify after the manner of girls in Germany by taking unto herself a lover with whom she cohabited for over twenty years. There was no true vagina, but the cohabitation was attended by the discharge of a thin viscid fluid from the urethra, and she stated that sexual excitement always brought on a peculiar thrill, a glow on the *left* side of the pelvis, the significance of which statement is explained by the anatomical evidence.

In her nineteenth year a discharge of blood took place from the urethra, which returned at irregular intervals for some time, and finally reappeared every three to four weeks, lasting freely from three to six days. This periodical sanguineous discharge was considered by Hohmann and her companions to be menstruation, and was preceded by the usual menstrual phenomena, such as tumefaction of the breasts, easy erectibility of the nipples, and between the twentieth and thirtieth years the secretion of a colostrum-like fluid from the latter.

After the advent of menstruation the relations of Hohmann with the male sex in general, and her lover in particular, remained about the same as before. As she approached the fortieth year the menstrual flow gradually decreased in duration to two days and the discharge of colostrum from the breasts at the monthly periods ceased entirely. From this time the tendency of the sexual passion changed and the propensity to cohabit with the female sex was developed and ultimately gratified.

Dr. Reder, a physician in her native town, was the first person who discovered the anomalous construction of Hohmann on an occasion of her consulting him for a supposed crural hernia. She refused at first to submit to any special medical examination, but in 1866 she concluded to present herself to the medical authorities of Würzburg for a thorough examination. The result of this examination is thus described by Dr. Mundé.



She was first seen by Professor von Recklinghausen, who, on examining her, found blood oozing from her urethra, for, as she states, she "happened to be menstruating when she came to Würzburg." Having been informed by Dr. Roder of the undoubted ejaculation of seminal fluid from the same urethra, Recklinghausen was naturally much interested, and referred her to Scanzoni and K lliker for further investigation. After keeping her under close observation in the Lying-in Hospital for two months (the personal supervision being exercised by Dr. von Franqu , who kept her in almost solitary confinement to prevent all chance of deception), these three professors made the following report of her sexual condition:—

"The person baptized by the name of Catherine Hohmann, of Mellrichstadt, is an exceedingly interesting case of hermaphroditism. The external genital organs have generally a masculine type: on the right side a distinct testicle and scrotum, a penis with moderate hypospadiasis, a urethra  $3\frac{1}{2}$ " in length; in the left inguinal region a soft, apparently somewhat lobulated body, which it is very questionable whether to consider as a second testicle; a body situated to the left of and behind the penis is likewise too soft and indistinct to be taken for a normal testicle. From above the root of the penis descend two sinuous folds of integument, which pass behind the corona glandis to the hypospadic frenulum, which has two gaps at its point of insertion. The breasts are of female type, fully developed, as well as the nipples and areol , the latter studded with hairs. A distinct beard is present. An examination per rectum did not reveal a rudimentary uterus. Should it be proved that neither of the two tumours perceptible on the left side are male sexual glands, this case might possibly be one of the rare cases of so-called lateral hermaphroditism. At all events, the observation that male and female functions are both present is of the highest interest. A fluid taken from her in 1863 and examined by Dr. Vogt showed the presence of spermatozoa; we, the undersigned, repeatedly observed the discharge of blood from the urethra, lasting two days, and presenting, by means of the perfectly fresh character of the corpuscles and the admixture of mucus, a menstrual appearance."

From Wurzburg Hohmann went to Heidelberg, and spent some time in the clinic of Friedreich. This distinguished observer also wrote a special report on her case which tallies with that we have given above, but with some additions that are deserving of notice. He held that with the exception of the hair of the head and the mammae nearly the whole formation of the body had a masculine type. "The larynx, the muscular development, thorax, pelvis are decidedly masculine; the type of inspiration is diaphragmatic. The existence of a short urethra also tends to place it in the male category, since the hypospadiasis is not total.

"With a sound," he adds, "I was able repeatedly to discover a pocket-like cavity at a distance of 1" from the urinary meatus, in which cavity the finger in the rectum could easily distinguish the point of the sound. The introduction of the sound was easiest

when the penis was held upwards, stretched, and the sound passed along the posterior surface of the urethra. There seems to me no doubt, therefore, of the presence of a thin-walled uterus masculinus."

A complete examination was next made by Professor Schultze, of Jena, and afterwards by Rokitsansky and Virchow. Of these, Schultze's report leaves some doubt as to the existence of an ovary. Rokitsansky calls the case "the first case of real, true bisexuality in man." He agrees with the description of the internal sexual organs given by Schultze, and says that, while in most cases of almost total absence of the uterus the ovaries are rudimentary, in this instance the ovaries appear to possess a more perfect development and to contain Graafian follicles, as evinced by regular menstrual discharges. Therefore, both kinds of sexual glands are united in this person.

The opinion of Virchow is very decisive. He did not succeed in touching the left ovary, but he speaks of the case as "the only instance, to his knowledge, in which the characteristics of true hermaphroditism have been traced so closely as to require only the anatomical confirmation of the conclusions already arrived at."

Hitherto, Dr. Mundé states, Hohmann had still passed publicly for a female, and had worn female attire, changing it for the male costume on her travels, or alternating between the two during her professional exhibitions. In 1872, however, there being no female function left, her name was legally changed to Carl, and she definitely assumed the male station. Soon afterwards he married, and in October of last year went with his wife to America, where he called on Dr. Mundé. Dr. Mundé had seen this person eight years previously in Germany as the woman Catherine Hohmann. Now he saw the same as the man Carl Hohmann, a married man, "a sleek, gentlemanly-looking person in a clerical suit of black." A person very much changed for the better in a masculine point of view. After describing many details of the condition of the subject of his observation at this time, the author adds: "I could no longer pass a probe into the rudimentary vagina and uterus, as I had done in 1868, which Hohmann accounted for by saying that in Kiel, a few years previously, the urethra had been probed with so much violence as to cause profuse hæmorrhage, and that since then no one had been able to enter the vagina. Probably it had been closed by adhesive inflammation. Neither could I detect, therefore, the slight enlargement formerly recognised as a rudimentary uterus. Agreeing with all previous observers, I found no trace of a prostate gland or vesiculæ seminales. Of a right ovary, or anything that might be taken for it, I could discover no trace whatever."

In concluding his monograph, which in a forensic sense is as remarkable as it is in a biological, Dr Mundé remarks: "To what conclusion now does a careful and impartial review of the anatomical relations of Hohmann lead us? Is *he* a man, is *she* a woman, or is

it really a true hermaphrodite? I am afraid this is a question which cannot be answered conclusively in all its points at present. A man, and only a man, he certainly is now, and has been ever since the alleged menopause; this he admits, and there would be no use in his attempting to deny the fact, for it is obvious to every observer. Therefore, being a man, he evidently cannot be a woman at the same time, unless the sexual qualities of both are united in one body, and this the early history of Hohmann would appear to render probable.

"The observance by competent and reliable authorities of a regular periodical discharge of blood from the genital organs, preceded by the ordinary menstrual molimina, is certainly the strongest point in favour of the existence of a female germinal gland. Menstrual *molimina*, at least, have been observed only where there was or had been an ovary (as after some double ovariectomies): so-called pseudo-menstrual sanguineous discharges have nothing to do with ovulation, and do not recur for years (24), as in this case. Granting the significance of this symptom as an evidence of the presence of an ovary, the undisputed contemporaneous emission of seminal fluid at once decides the question in favour of true hermaphroditism."

Altogether the author is of opinion that the view expressed by Virchow, above mentioned, is the most clearly and fairly expressed.—*Reprint from the 'American Journal of Obstetrics and Diseases of Women and Children,' February, 1876.*

### III. HYGIENE.

*Female Dress. Fashion and its Penalties.*—Dr. Washington L. Atlee discourses well on this subject. "Beauty and health are twin sisters." "Examine these two beings under another aspect. Place your line on the mastoid process of the one, and the plummet, as it should do, will strictly indicate the axis of the spinal column, and strike the malleolus. This, therefore, is the centre of gravity, and its force does not impinge upon a single vital organ. Make the same experiment with the other. The line of gravity will be different; it leaves the vertebral axis, and passes through the organs of the chest, the viscera of the abdomen, and impinges upon the pelvic organs. Add to this a waist contracted and rigidly fixed. Now what must be the result? The upper wall of the thorax, being an unyielding cone, and its lower border being incapable of expansion, the only compensation must be in the direction of the least resistance. We all know where that is. There are from twenty to forty inspirations every minute during every day of our existence, and in such a faulty attitude of the body the diaphragm is driven down by each inspiration; in the line of the centre of gravity, like the piston-rod of a pump, forcing every organ below it more or less out of place. All violent and unusual exertions of the body also act in the same direction.

"Is it any wonder, therefore, that the diseases peculiar to females should have increased to so alarming an extent? And is it strange

that, with all these counteracting causes, we should find these ills so difficult of cure, and, when cured, so apt to return? It is fortunate for women, amidst the follies of dress and the foibles of fashionable society, that pathology and treatment have made so much progress in uterine troubles. Were we not in advance of the knowledge of old physic, and did we possess no better means of combating the destructive influence of the times, our households would become female hospitals, and the treatment of such diseases would be an opprobrium to medicine. But to do the best we can, this 'patched-up' existence is but a poor substitute for that buoyancy of health and spirits which is the natural birthright of the majority of women. I have often said to my lady patients privately, and, gentlemen, I say it to you publicly, that if the ladies of this country, instead of being travestied by milliners and mantle makers, and enslaved by every change in the tide of fashion, would, before adopting them, submit their costumes to a committee of medical men, or better, of medical women, they would be infinitely more comfortable, would enjoy better health, more satisfactorily fulfil the duties of matrimony and of marital life, and meet the requirements of every domestic and social position. We certainly would recommend no more clothing than could be carried with ease and comfort; we would suspend all garments upon the shoulders; we would not constrict the most important part of the body, making that portion of the chest which is naturally the most expansive a contracted, immovable *point d'appui* for every inspiration to drive down and displace the vital organs; we would have the shoe to fit the foot, not forcibly adapt the foot to the shoe; we would order the heels to be low and broad, and placed where the Almighty designed them; we would discard furs from the neck and shoulders for common use, reserving them for extraordinary occasions, and *veto* the use of unwieldy masses of false hair—as these portions of the body are so near the centre of circulation as to have their heat well maintained; in short, in adopting any style of dress we would do no violence to the laws of physiology and hygiene. This could always be accomplished in perfect harmony with good taste. The health of woman, which is so intimately associated with the beauty, welfare, and happiness of the whole human race, is too valuable to be sacrificed to the blind and indiscriminating tyranny of fashion."—*The Sanitarian*, April, 1876.

*The Soil in relation to Disease.*—Drs. T. B. Lewis and D. D. Cunningham, special assistants to the Sanitary Commissioner with the Government of India, have drawn up a report of observations which have been carried out with a view of determining to what extent peculiar conditions or changes in condition of the soil in Calcutta affect the prevalence of disease in general and of certain diseases in particular. The subjects specially investigated were: (1.) The amount of moisture in the soil. (2.) The temperature of the soil. (3.) The amount of carbonic acid in soil. Some of the observations made were at the suggestion of Dr. von Pettenkofer. After describing in detail the processes they followed out, the

authors drew a comparison of the prevalence of disease with the occurrence of various conditions of soil in regard to carbonic acid, temperature, and water-level. These facts are briefly summed up as follows.

*Prevalence of Cholera.*—The only remarkable coincidence appears to lie in the converse relation which water-level, and in a less marked degree rain-fall, bear to the prevalence of the disease. When the latter is at a maximum, the water-level is at a minimum, and when the water-level is at a maximum, the prevalence of cholera is at a minimum. There is no such close coincidence either in regard to conditions of soil-temperature or amount of carbonic acid, although, in so far as soil-moisture appears to determine the amount of carbonic acid in the soil, there is a general coincidence in regard to the latter also. The relations between rain-fall and prevalence of cholera are not so strongly marked as those between the latter and the water-level; and it even appears as though the converse relation between conditions of water-level and prevalence of cholera were in some degree more distinct than the direct one between the water-level and the rain-fall.

*Prevalence of Fevers.*—The greatest prevalence of fevers during the period of observation occurred coincidently with the period of maximum carbonic acid and highest water-level.

*Prevalence of Dysentery.*—There were two maximum periods of dysentery, one occurring during the rise in the water-level, and the other at a corresponding point in the course of its fall. No coincidence can be traced in regard to the other conditions of soil, save the carbonic acid of the upper layer which in this part of its course very closely corresponds with the water-level.

*General Mortality.*—No very clear connection can be traced between the statistics of total mortality and the prevalence of any special conditions of soil. There were two periods of maximum mortality during the period of observation—one in November and December, coincident with marked prevalence of fever and dysentery; the other in April and May with maximum cholera.

#### IV.—SUMMARY.

*Thymol and Phenol as Poisons.* By T. HUSEMANN ('Archives of Pharmacy,' iii, 7, pp. 228).—Dr. Husemann treats on the toxic action of poisons with their atomic weight. He compares the salts of potassium and lithium in respect to their toxic action, and disputes Rabuteau's hypothesis, which rests chiefly on our own observations respecting the alcohols, that the toxicological effect of the metals increases with the increase of their atomic weight. He admits the correctness of our demonstration, that the toxic action of the alcohol is greater the higher the molecular weight; but in other senses he says the opposite is the case. Of this thymol and phenol are examples. Thymol has a much less energetic action than phenol. It has no irritating action on the skin, but it irritates the mucous membrane of the mouth. On rabbits it causes, in large

doses of thirty grains, no further effect than a slight increase in the pulse and a slight decrease in the respiration and temperature. Carried to a poisonous action, it causes an increase of fat in the liver, congestion of the kidney, excretion of albumen and blood by the kidney, and gradual paralysis. Phenol produces none of these distinct effects.

*Sodium Amalgam—method of detecting Arsenical Compounds.* By Mr. EDMUND DAVY ('The Chemical News,' No. xxxiii).—Mr. Davy suggests a change in the carrying out of Marsh's process for the detection of arsenic. He objects to the employment of zinc and sulphuric acid in Marsh's method on the ground that both these substances may contain the poison. He uses instead an amalgam of sodium and mercury. Sodium never contains arsenic, and mercury may always be freed of it by digesting in strong nitric acid, and afterwards washing in water. He proceeds by making an amalgam composed of part by weight of sodium to eight or ten parts of mercury. The mercury is treated in a test-tube, and the sodium gradually added in small portions; the metals easily combine, forming an alloy whilst hot, but hard and brittle when cold. In testing Mr. Davy places the suspected substance or solution in a test-tube with a little water. To this he adds small pieces of the amalgam, about the size of a grain of wheat, and quickly covers with a piece of white filtering paper, or the lid of a porcelain crucible moistened with a dilute solution of silver nitrate—twenty grains of silver nitrate to an ounce of distilled water—acidified with two drops of strong nitric acid. When arsenic is present a dull black or deep brown stain is developed on the paper or porcelain at the moistened part, owing to the reduction of the silver to the metallic state by the arseniuretted hydrogen.

*The Endocardium after Arsenical Poisoning.*—By ROBERT HARVEY ("Report on Medico-legal Returns," 'Indian Medical Gazette,' April 1st, 1876).—Mr. Harvey, in this report, notices a fact which has not previously been made so fully known as he puts it, viz., the condition of the endocardial membrane after poisoning by arsenic. The author has collected together the facts relating to a very large number of arsenical poisonings, and on the subject now specially named he reports as follows:—

"The condition of the lining membrane of the heart is given thirty-three times, and is a point which should be carefully noted in future. In eight cases only in which it was carefully examined was it found to be natural. In two cases by Dr. Warburton the endocardium was deeply stained, but the action of running water rapidly removed the congested appearance, which does not seem to have amounted to ecchymosis.

"In twenty-three cases it was found congested or ecchymosed, the appearance being apparently confined to the left ventricle in most of them. The mitral valve was stained with bluish spots of extravasated blood in a case reported by Dr. Harris, of Shahjahanpur, in February, 1870. This officer reports seven cases of arsenical poisoning in three years, in five of which this endocardial congestion was

found, and with reference to its absence in one of the cases of 1871, he remarks that, with this exception, he has always found it since his attention was directed to the subject. No endocardial ecchymosis, was, however, found in a second case in September, 1872, although much arsenic was found in the stomach, which was greatly inflamed and almost ulcerated. The duodenum also was inflamed, and the liver, lungs, and brain congested. In a case from Lucknow, in October, 1871, 'the heart was of normal size, its cavities were empty, rather reddish in colour, and marked with blackish patches;' and another in the December return, from the same station, presented similar appearances. In a third case, in the same year, the heart was found 'normal.' In a well-reported case by Sub-Assistant Surgeon Rajkishon Mookerjee, in the Gurdaspur return for March, 1871, the endocardium was marked with livid patches, which were more distinct on the columnæ carnæ of the left ventricle. The experience of Drs. Bonavia and McReddie is that the ecchymosed spots are most common near, but not on, the fleshy columns. Briand gives the valves and fleshy columns as the most frequent sites."

*On Testing for Fusel Oil.* By A. DUPRÉ, Ph.D., F.R.S. ('The Analyst,' March 31st, 1876).—At this time, when so much is said respecting the presence of fusel oil in common ethylic alcohol of wines and spirits, the mode of testing for the oil invented by Dr. Dupré deserves special record. The description of his process is as follows:

"Fusel oil, as is well known, consists of a mixture of various of the higher homologues of ethylic alcohol, all or most of which, when oxidised by means of sulphuric acid and potassium dichromate, yield their corresponding acids and these latter are much more readily separated than the alcohols.

"Upon this fact the method is based. An amount of spirit containing from one to two grammes of alcohol, previously distilled if necessary, is oxidised in a closed flask by means of sulphuric acid and potassium dichromate, care being of course taken to have an excess of this mixture in the flask. I usually digest the mixture in the flask for two hours in a water bath.

"When cool the flask is opened, the excess of dichromate present reduced by zinc, and the acids produced are distilled off (see the work previously quoted). The acid distillate is now neutralized with a standard solution of normal soda, the solution is evaporated to a small bulk and transferred to a retort. An amount of normal sulphuric acid equal to one twentieth of the normal alkali used is now added, and the contents of the retort are distilled to dryness in an oil bath; the temperature being allowed to rise to about 180° C. Water is now added and a further addition of one twentieth proportion of normal acid is made, after which the contents are again distilled to dryness. These two distillates may be collected separately, but I prefer to collect them together. It is advisable to add some water to the dry residue in the retort, and again distil to dryness, repeating this addition of water and distilla-

tion three times after the second addition of acid. The acid distillate which contains all the acids higher in the series than the acetic acid, together with a proportion of this latter, is now neutralised by means of pure carbonate of barium, the solution is boiled, filtered, evaporated to dryness, the residue dried at 180° C., and weighed.

"The amount of barium contained in the salt is now estimated in the usual way by conversion into the sulphate. We now have the necessary data for calculating the amount of fousel oil contained in the spirit under examination, on the assumption that it consists either of amylic alcohol or of any other alcohol that may be supposed to be the chief impurity present. The real amount present cannot of course be obtained without a knowledge of the exact nature of the acids produced, but even this can be accomplished according to Ducloux ('Compts. Rënd.,' lxxviii, p.1160), by submitting the mixture of acids to fractional distillation, and estimating the proportion of acid which does over with each fraction."

*Registration of Infectious Diseases.* By Dr. EGELING ('Proceedings of the National Association for the Promotion of Social Science,' April 29th, 1876, vol. ix, No. 12).—A paper read by Dr. A. Carpenter before the Association, on 'The Right of the State to obtain Early Information on the Appearance of Epidemic or Infectious Diseases,' led to a very valuable discussion. In the course of the proceedings Mr. Adam Scott read a letter from Dr. Egeling, of the Hague, on the mode in which such information is legally collected in Holland. The information is valuable at this time, because it may indicate the probable process that will be carried out, in course of time, in this country.

"(a) In the law that regulates the practice of medicine (1865), the 6th article says, 'They (i.e. the medical men) are to give immediately, and at least within three days, notice to the medical inspector of their province, and to the mayor and aldermen whenever they observe a disease that is dangerous to public health.'

"(b). The 19th article of the law commands that the head of the family, the keeper of a common lodging-house, the captain or master of a ship, the director of an institution of public charity (orphan houses, poor houses, hospitals, &c.), or of a prison, &c., in which a contagious disease occur, must give notice of it to the mayor within twenty-four hours after the existence of the contagious disease comes to their knowledge.

"According to article 20 the houses in which such a disease occurs are to be marked by the authority of the mayor with a paper on which the words *contagious disease* and the name of the disease are printed in large letters.

"By article 14 the inhabitants of a house, or of a vessel in which contagious disease occurs are prohibited from going to school within eight days after the disease has ceased to exist in the house or vessel, according to a written declaration of a medical man. When *within* these eight days the house, &c., has been thoroughly disinfected, according to the rules given in a bye-law, the prohibition ceases.



The article does not bear upon schools in which there are only children above twelve years of age when the disease is measles or diphtheria.

"By these two laws there is a double notice given to the authorities, one by the medical man and one by the head of the family, and there is a punishment for omission as well for the medical man as for the head of the family."

*Ammonia as an Antidote for Snake Poison.* By V. RICHARDS ('Indian Medical Gazette,' April 1st, 1876).—Mr. Richards, civil surgeon, Goalundo, reports on the snake-bites which occurred in Bengal, Behar, Orissa, Assam, &c., during the years 1873-4. The returns show 4202 cases furnished from fifty-two districts. From the series of interesting and valuable facts epitomised in this report we extract as the most practical the author's observations on the treatment by ammonia. Of the 850 persons who were supposed to have been bitten by poisonous snakes, and were treated with ammonia, 440 were men, 355 women, and 55 children. The recoveries are said to be 606 or 71·30 per cent.—329 (74·77 per cent.) of the men, 252 (70·98 per cent.) of the women, and 25 (45·45 per cent.) of the children. The deaths were 240, 107 being amongst men, 103 amongst women, and 30 amongst children. The result was unknown in four cases. Dr. Halford's "cases" of the ammonia treatment by intravenous injection; Dr. Weir Mitchell's cases of Rattle-snake-bite, in which various modes of treatment were resorted to; and the Indian Commission's experiments with Australian snakes, in which no treatment was adopted—all show a percentage of 75 recoveries. This fact is, perhaps, no more than a strange coincidence, though it apparently demonstrates pretty clearly the utter uselessness of any particular mode of treatment. The statistics on which the above computation, viz., 71·30 per cent. of recoveries, is made, are not more untrustworthy than are those on which some experimenters have founded the reputation of the remedies they advocate.

The remedy was resorted to in 43 districts, but principally in the following:

Midnapore	..	119	with 90	..	75·63	per cent. recoveries.
Hooghly	..	56	" 45	..	80·35	"
24-Pergunnahs	..	49	" 35	..	71·43	"
Ranchee	..	45	" 38	..	84·44	"
Burdwan	..	40	" 23	..	67·50	"
Bhaugulpore	..	38	" 26	..	68·42	"
Jessore	..	37	" 24	..	64·86	"
Howrah	..	34	" 22	..	64·70	"
Balasore	..	32	" 25	..	78·12	"

Mean of the 9 districts, 71·71 per cent.

If we blindly accept these figures as correct, they do not prove the efficacy of the treatment, for while the recoveries, as stated in these statistics, range from 57 per cent. to 83· per cent. they average only 71· per cent. and other data may be adduced showing a higher,

rate of recoveries, by different modes of treatment. On analysing these data, however, we shall find that the average percentage of recoveries—amongst those cases, of the truth of which there is some internal evidence—is very insignificant, and can be accounted for on other grounds than the internal administration of ammonia.

There are cases on record of the utter failure of the intravenous injection of ammonia as a remedy in snake-poisoning in man.

“*Case.*—A Sepahi, named Ramnah, a Hindu, aged 28 years, was bitten by a snake (not identified), on the evening of Nov. 15th, 1874, while on the line of march with his regiment from Morar. He was bitten on the fourth toe of the left foot, and was taken to hospital at midnight, about  $4\frac{1}{2}$  hours after receiving the bite, when he was unable to walk or articulate, though his mental faculties were clear, and he could hear what was said to him. There were spasms of the whole body, and a swollen and painful condition of the foot. Saliva was flowing from his mouth, and the pulse was slow, weak and intermittent. Brandy and ammonia were administered internally, and ten minims, and subsequently fifteen and twenty, were injected into the median basilic vein, but he died shortly after the third injection. This case “came under the treatment of Surgeon F. W. Wright, M.B., who was then in medical charge of the 34th N. I.”

“When once a fatal dose of snake-poison is absorbed into the circulation, all our efforts to counteract its deadly effects are vain. The only rational mode of treatment appears to be that which has for its object the prevention of the absorption of the poison, with a minimum of injury to the bitten part.”

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## IN MEMORIAM—DR. PARKES, F.R.S.

It is with profound grief, shared not only by ourselves but by the whole medical and scientific world, that we sit down to write a short memoir of Edmund Alexander Parkes, who died at his residence, Bitterne, near Southampton, on the 15th of March, having nearly completed his 57th year. He survived the death of his wife, which took place in 1870, but he has left no family. Independent of the fact of his having been for some years Editor of this 'Review,' and one of its most valued contributors, his useful and lasting labours in the cause of humanity entitle the memory of this great and good man to more than a passing notice.

Short and fragmentary accounts of his life and works have already appeared in the medical journals of this country and the Continent; but we want more—we want a detailed account of the unselfish life and labours of a man, who during his career commanded the respect of the civilised world and, at his death, universal regret. We must express a hope that some personal friend who knew him well will write his biography for the benefit of future generations, or even as a stimulus to the student of medicine; but at any rate his writings will remain as monuments of patience, industry, and learning, and they will proclaim his greatness.

Dr. Parkes, though he did not often suffer from serious illness, never was physically strong; but what he wanted in body he made up for in a mind which possessed rare powers of discrimination and peculiar fitness in prosecuting original research. He was born at Warwick on the 30th of March, 1819, of parents possessed of those upright qualities which distinguish the Christian character—qualities which became farther evolved and perfected in their offspring. Parkes, though a patient and industrious investigator, was an example of hereditary genius; for his mother, we understand, was an authoress of several popular works, and no doubt in early youth he learned to cultivate those characters, literary and scientific, under the able guidance of his parents, which in after life raised for himself a name as the founder of Hygiene—a name destined to become famous in the history and practice of Medicine. He received some of his early education at that excellent institution, Christ's Hospital, which has given to the world many men who have distinguished themselves in various walks of life; but none more so than the subject of this memoir. He obtained his medical education at University College and passed through the various classes with distinction, taking both medals and other honours; in fact, while a student at College, he reflected his after greatness. He took M.B. in 1841, gaining honours in almost all the subjects of examination, and his M.D. in 1846.

While at University College he assisted his uncle, Dr. Anthony Todd Thompson in his Laboratory, and no doubt he there became

imbued with those habits of accuracy and research, especially in chemical investigations, which distinguished him to the end of his days. It may not be generally known that the Army Medical Department had the honour of claiming him as one of its own, at an early period of his career; but he did not remain long in that service. In 1842 he was gazetted Assistant Surgeon to the 84th Regiment, in which he served for three years in Madras and Burmah, and where by steady and close observation he became acquainted with the nature of those tropical diseases, especially cholera, dysentery, and hepatic affections, so perfectly that, soon after his arrival in England, he was induced to publish an account of each. He returned to England in 1845, resigned his commission and settled down to private practice in London, where he resided for nearly ten years.

During the period of his residence in London he was able to publish, besides attending to his private practice, numerous papers and pamphlets of great value, notably 'Remarks on the Dysentery and Hepatitis of India,' and 'On Asiatic and Algide Cholera,' besides papers and contributions to the medical journals. In 1849 he was elected Professor of Clinical Medicine to University College and Physician to University College Hospital—a proper position it will be admitted for a man of his unusual attainments to occupy; and it was there, at the patient's bedside, his great powers of observation and teaching first became apparent—powers which gathered, as it were, increased strength and charms with his advancing years. We who have been his pupils at a later period, and who have known him well, cannot easily forget one to whom we owe so much either as a faithful teacher, as a man or a true friend.

In 1851 he edited a new edition of 'Thomson's Diseases of the Skin,' a subject in which he took great interest.

In 1852, in addition to his other duties, he became Editor of this 'Review,' and continued in that capacity for three years, when, during the Crimean War, he was selected by Government to organize and superintend a hospital in Turkey for the relief of our sick and wounded. The confidence of the Government in him was not misplaced and he eventually chose Renkioi, on the Dardanelles, where a large hospital was constructed and managed in the most perfect manner. From that time he may be said to have formed his second connection with the Medical Department of the Army, and to have commenced that career of usefulness in Hygiene especially which has led to the improvement in the sanitary condition of all classes, more especially the soldier; and which connection was continued until his comparatively early death. In 1855 he delivered the Gulstonian lectures on pyrexia, afterwards published in the 'Medical Times,' and he was appointed in the same year Examiner of Candidates for the Indian Medical Service, and subsequently for the other Public Medical Services—positions for which he was specially qualified and which occupied a great deal of his time and attention. In 1860 he was selected Professor of Hygiene for the

Army Medical School, Fort Pitt, and in the same year he published a work on the 'Composition of the Urine in Health and Disease.'

The high rate of mortality and sickness of our troops, from preventable causes, during the Crimean War, and always in India, and the want of special knowledge in Military Medicine, Surgery, and Hygiene amongst the medical officers of the Army, were, no doubt, fully observed by Dr. Parkes in the Crimea. To his labours, therefore, and to those of the late Lord Herbert, we are principally indebted for the Army Medical School, which was removed from Chatham in 1868 to that more spacious and elegant edifice the Royal Victoria Hospital, Netley, and where instruction in all the above specialities has been since given to all departments of the Public Medical Services.

The means of preserving health and the prevention of disease as taught by Parkes at the Army Medical School soon began to excite attention, and consequently at the recommendation of Sir James Gibson, K.C.B., the Medical Director-General, he published in the 'Annual Departmental Volumes' a Report on Hygiene, collected from the various published works bearing on the subject, whether in this country or abroad. These Reports, which commenced in 1862 and were continued up to near the time of his death, are monuments of industry, the results of careful thought, extensive reading, and research, and were always most anxiously looked for by army medical officers all over the world, as well as by all those interested in the progress of Hygiene. He delivered the Introductory Lecture at the opening of the Winter Session of the Army Medical School at Fort Pitt, on the 1st of October, 1862, which was afterwards published in the 'Lancet.' In 1864 he brought out his great work on Hygiene, which he specially prepared, in order to carry out the wishes of the Royal Commission on Army Sanitation, &c., by providing a text-book of Hygiene illustrated by examples drawn from army life, for the gentlemen attending the Army Medical School.

That book has since passed through four editions, so great has been the demand, and at present it is so arranged that it forms a complete guide to Hygiene adapted to all who are entrusted with the duties of the various departments of public health. It has, moreover, been translated into many languages; it is the text-book of Hygiene in America, and is as well known perhaps in France and Germany as in this country.

In 1868 he published a 'Scheme of Medical Tuition,' which appeared in the 'Lancet,' and which should be read by every one engaged in the instruction of Medical Students. Amongst other things in the 'Scheme' he recommended compulsory attendance of the students at the patients' bedsides, where they would be taught by the teacher and made to examine each case for themselves, in accordance with the system introduced by Dr. Gairdner, at Glasgow; in fact, he impressed the necessity of their becoming practical professional men instead of useless theorists.

His broad and enlightened views on Medical Education often found expression in the occasional lively debates of the General Medical Council, of which he was one of the principal members, and it must be said that when he spoke in that assembly he always, by his good sense, commanded the respect and goodwill of his colleagues who might differ from him.

He published various papers which appeared in the Proceedings of the Royal Society in 1867 and 1871, 'On the Elimination of Nitrogen during Muscular Action,' and again in 1872 and 1874 'On the Effects of Alcohol on the Human Body,' besides numerous other papers which appeared in the various medical journals.

He was a powerful advocate of the Contagious Diseases Acts, because he realised the benefits that would result—to all classes—from their successful application; and he always met the opponents of the Acts by crushing arguments and incontrovertible statistics as to their value.

He worked hard for the benefit of the soldier, and assisted in introducing many reforms in his dress and equipment, so as to enable him to preserve his health and efficiency at the same time.

What greater proof is needed of his world-wide and philanthropic labours than the eloquent tribute recently paid to his memory by Baron Mundy, of the Austrian Army, and published in the 'Wiener Medizinisch Wochenschrift' for April. After alluding to his loss, Baron Mundy says: "Every Continental army should, were it only for a moment, dip its crape-clad standards on parade, in memory of the founder and the best teacher of military hygiene in our times. The friend and benefactor of every soldier, Edmund Alexander Parkes, is no more!"

In a short notice like the present it is impossible even to enumerate the labours of Dr. Parkes, much less to give any idea of their nature. He was always busy; it was natural to him, and he made the most of his time, not for his own benefit, but for the good of others. Shortly before his death he wrote a popular work on Hygiene, entitled, 'On Personal Care of Health,' which is now published under the auspices of the Society for Promoting Christian Knowledge, and it will, no doubt, do an immense amount of good if attention be paid to the simple rules laid down in the plain and easy language of its lamented and gifted author. He was one of those unobtrusive men who does good, as it were, by stealth, and who makes his services subservient to the advantages of others. His life of usefulness, devoted to the improvement of mankind, and especially his services to his country, called loudly for some recognition during his lifetime; but he has passed away without a state reward or distinction, happy in having done his duty to his fellow-man, and in having earned the affectionate love of his numerous friends, and of all those who ever knew him.

We are glad, however, to learn that it is intended, by his former colleagues at University College, to establish a permanent record to his memory, and of such a character as to aid in the scientific inves-

tigation and practical study in the subjects in which his life was chiefly spent. We also know that a movement has commenced at Netley, having for its object, amongst other things, testifying to the esteem in which he was held, the foundation of a Parkes' Scholarship or Prize, to be competed for in the Army Medical School.

At Netley, the scene of his principal labours, his loss will be severely felt; both in the laboratory, where his earnest, practical, gentle manner of teaching always commanded the attention, admiration, and respect of the students, and in the Senate, where his wisdom and experience were so useful in conducting the business of the school. We hear nothing but lamentation since his death, and we have listened to more than one of his distinguished Colleagues who have paid his memory the highest tribute that could be accorded to any man, and one of whom very beautifully expressed himself regarding his scientific attainments, that "his name runs like a golden thread through the woof and web of modern medicine."

Dr. Parkes was no theorist—he died without a theory; he was practical, minute, and accurate in all his investigations; he had an abhorrence of oppression and wrong; he was, in fact, judging from his public and private life, the nearest approach to perfection in a man and a Christian that it is possible to attain; and of him it may be truly said that he had "a soul exalted above earth—a mind skilled in the arts that form mankind."

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THE  
BRITISH AND FOREIGN  
MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1876.

Analytical and Critical Reviews.

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I.—The Physiology of the Vaso-motor System.<sup>1</sup>

A QUARTER of a century has elapsed since Bernard published the first results of his enquiry into the effects produced by division of the cervical sympathetic in the rabbit. During this period of five-and-twenty years a vast amount of conscientious labour has been spent upon the study of the complex relations between the nervous and the vascular apparatus; many new facts have been observed and added to the permanent stock of our knowledge; finally, a consistent theory of vaso-motor action has been gradually built up. The pathologist, the pharmacologist, the practical physician, ever on the *qui vive* for experimental data or hypotheses capable of throwing light on the innumerable problems continually—and for the most part vainly—calling for immediate solution, have grasped at every new speculation as it issued from the laboratory. The mystery of many a morbid process, the peculiar action of many a remedy, the connection between seemingly incoherent symptoms, have often seemed clear or on the verge of becoming so, as some fresh generalisation concerning vaso-motor nerves has been brought before the world. The dominant theory of vaso-motor action has been the sword wherewith many a Gordian knot has been severed. Such being the nature of the relation between our knowledge of the vaso-motor system and all the more important branches of medicine, it seems desirable to survey the ground from time to time,—to ascertain the value,

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<sup>1</sup> *Influence du grand Sympathique sur la Sensibilité et la Calorification.*  
'Comptes Rendus de la Société de Biologie,' 1851.

both as regards quantity and quality, of contemporary information in this department of physiology,—to find out whether it is really sufficiently precise, certain, and coherent, to serve as a groundwork for the various hypotheses, pathological and therapeutical, that are being founded upon it day by day.

Although it may fairly be said that the attention of physiologists was first drawn to the vaso-motor function of the sympathetic by Claude Bernard, in 1851, it would be a mistake to suppose, either that his fundamental experiment had never been performed before, or that the relation of the arteries to the nervous system had not been suspected by his predecessors. Bernard himself refers to a paper read before the Academy of Sciences, in 1727, by Pourfour du Petit, in which some of the chief effects of cutting the sympathetic in the neck of a dog were clearly described. Petit's choice of an animal was unfortunate for two reasons: in the first place, the cervical sympathetic of the dog cannot be divided without at the same time dividing the vagus, and thus running the risk of ascribing the phenomena produced to a wrong cause; secondly, an increase of vascularity in the ear of a dog does not attract attention as it does in the ear of a white rabbit. As it turned out, Petit attributed the contraction of the pupil and the recession of the eyeball to their true cause, viz., solution of continuity of the sympathetic trunk; but he failed to perceive the simultaneous rise of temperature in the corresponding side of the head. Again, only a year before Bernard's researches were made public, Budge and Waller had repeated and confirmed Petit's observations; and, by employing the method of nervous degeneration with which their names have become associated, had proved that those fibres of the sympathetic to whose section the pupillary contraction was due, were derived from a particular part of the spinal cord, which they named the "cilio-spinal region." But the discovery of the congestion and rise of temperature was still reserved for Bernard's acumen.

The presence of contractile—as opposed to elastic—elements in the walls of the arteries was known to John Hunter; but Henle was the first to describe those elements (in 1840), and to demonstrate their identity with the unstripped muscular fibre in other parts of the body. It is true that, even before Henle, the middle coat of an artery had been termed its "muscular coat;" but this designation was based on mere hypothesis. To Henle belongs the credit of having actually proved the existence of fusiform contractile elements in the middle tunic of the arterioles and veins.<sup>1</sup> Moreover, he clearly perceived the

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<sup>1</sup> 'Wochenschrift für die gesammte Heilkunde,' 1840, No. 21.

proper function of arterial contractility, that function which has since been happily described as the "stop-cock action of the small arteries." The share taken by the heart, and that belonging to the smaller vessels, in the circulation of the blood, may, he says, "be stated in a few words; the movement of the blood depends upon the heart, its distribution upon the vessels." He also realised the truth that the nervous system might exercise a powerful influence upon the muscular coat of the vessels. About the same time, too, Stilling concluded on *a priori* grounds that the calibre of the vessels might be regulated by nerves, either directly or by way of reflex action. To him we owe the term *vaso-motor*. He contrived it in order to indicate that these vascular nerves were in all respects analogous to the *musculo-motor* nerves governing the contractions of the voluntary muscles.

Correct as these speculations have turned out to be, they were only speculations after all. Function may be inferred from structure; but the inference can only be viewed as provisional until it has been verified by direct experiment. Singularly enough, even the ingenuity and probability of these speculations failed to excite any very lively interest in the scientific world. So that the popular voice is, upon the whole, not unfair in ascribing the credit of the entire conception to M. Bernard.

In his first communication to the Academy of Sciences,<sup>1</sup> this physiologist was content to state the facts he had observed, viz., that in addition to the phenomena described by Petit, section of the cervical sympathetic was followed by congestion and a marked rise of temperature in the ear and face on the side corresponding to the operation. He did not attempt to explain the mechanism by which the vascular turbulence was brought about; and he was inclined to regard the elevation of temperature as in some measure independent of the increased activity of the circulation. It was left for Brown-Séquard<sup>2</sup> to show that the dilated condition of the vessels was really due to paralysis of their contractile walls, and to assign the rise of temperature to its true cause, *i. e.*, to the larger volume of blood sent through the affected area in a given time. He likewise proved that electrical stimulation of the distal end of the divided nerve was able to restore the dilated vessels to their normal calibre and simultaneously to lower the temperature of the congested part. The same results were independently reached both by Bernard and by Waller.

<sup>1</sup> 'Comptes Rendus,' 29 Mars, 1852.

<sup>2</sup> 'Philadelphia Medical Examiner,' August, 1852.

The attention of physiologists had now been thoroughly awakened, and not a year has since elapsed without bringing some contribution to the subject. Bernard himself has again and again returned to it; and among the physiologists of Germany, Ludwig and his pupils stand foremost in regard of the attention they have bestowed upon it. Differing not inconsiderably on many points of detail, there has nevertheless been enough of underlying harmony among the results attained by these various independent enquirers to justify the construction of a general theory of vaso-motor action, a theory which has been very generally accepted, and which has found its way into the more modern of our text-books. We shall in the first place, give a brief account of the growth of this theory; next, we shall consider how far it agrees with the facts now at our disposal, and whether it be not in great need of thorough reconstruction.

This is the place in which to say a few words about the principal methods by which we are enabled to estimate variations in the calibre of the small arteries, or, indeed, to ascertain that such variations occur. Direct inspection, with or without the aid of measurement, is obviously the simplest of these methods; but it is only applicable to parts within reach of the eye, as *e.g.* the ear of the rabbit, the frog's web, the wing of bat. When this resource fails us, we may compare the rate at which blood flows from a wound in a part where vessels are supposed to be either relaxed or contracted, with the rate of bleeding from a wound made in a corresponding part, whose arterioles are of normal calibre. When the vessels are contracted, the blood escapes very slowly, and is dark in colour; when they are dilated, bleeding is more profuse, and the colour of the blood is brighter. Thirdly, local elevation or depression of temperature affords proof, and, if necessary, quantitative evidence, of arterial relaxation or contraction. Next, the state of the blood-pressure in the arterial system is capable of affording information as to the state of the contractile vessels. The mean arterial pressure at any given moment depends, as is well known, upon two factors—the propelling power of the heart, and the resistance offered to the onward movement of the blood by the arterioles. Supposing the former of these elements to remain approximately constant, any deviation from the normal standard of mean arterial pressure will indicate contraction or relaxation of the smaller vessels, their contraction causing a rise, their relaxation a fall, in the mercurial column of the manometer. Lastly, we have a very ingenious instrument, to which its contriver, Dr. Mosso,<sup>1</sup> has given the name of *plethys-*

<sup>1</sup> "Von einigen neuen Eigenschaften der Gefässwand." 'Ludwig's Arbeiten,' 1875.

*mograph* (from *πληθύνω*), which enables us to recognise and measure trifling variations in the bulk of organs removed from the body, through whose vessels a current of defibrinated blood is being pumped (liver, kidney); the apparatus also admits of being modified in such a way as to measure slight alterations in the bulk of the limbs (arm, leg) during life; all such variations in bulk having been proved, *per viam exclusionis*, to depend on variations in the flow of blood through the capillary vessels of the part, and therefore on variations in the calibre of the smaller arteries.

Experiments with the mercurial kymograph led to the discovery that the mean arterial pressure in any animal, supposing it to continue in a state of health, is almost as constant as its temperature. It follows, therefore, that the degree of contraction of the arterioles must vary inversely as the propelling force of the heart. With the nervous mechanism by which this adjustment between the central and peripheral parts of the circulatory system is maintained we are not at present concerned. All that we need consider is the fact that the small arteries are habitually kept in a state of partial contraction through the agency of the vaso-motor nerves. This state may vary within relatively narrow limits, never approaching the extremes of complete relaxation or complete constriction, except under conditions that are altogether abnormal. It is to this habitual state of medium contraction that we give the name of "arterial tonus."

Let us now return to Bernard's fundamental experiment. It did not take very long to find out that a dilated or relaxed condition of the small arteries could be produced by cutting nerves, in other parts of the body, as well as in the face and ear of the rabbit. The great mixed nerves supplying the limbs were divided, and it was seen that the motor and sensory paralysis resulting from the operation was attended by increased vascularity and rise of temperature in the paralysed part. Section of the splanchnic nerves caused relaxation of the arterioles in the abdominal viscera, and by diverting a large proportion of all the blood in the body into the capacious reservoir formed by this very extensive and dilatable vascular area, diminished the resistance to the discharge of blood from the left ventricle to such an extent as to cause a great fall of systemic blood-pressure. All the cases investigated had one feature in common; whatever the character of the nerve by whose division a relaxed state of the vessels was induced, it was always found to contain fibres derived from the sympathetic—fibres topographically distinct from those conveying musculo-motor impulses and sensory impressions from and to the spinal cord.

Thus, for example, section of the sciatic trunk in the dog causes paralysis of motion and sensation in the hind limb, together with vascular congestion and rise of temperature. Bernard succeeded in analysing this group of phenomena and separating its two main divisions from each other. By cutting the lumbar nerve-roots from the fourth to the eighth, together with the first three sacral roots, in the vertebral canal, he succeeded in producing complete paralysis of motion and sensation *without* relaxation of blood-vessels. On the other hand, by tearing out the small ganglion which lies on the side of the body of the fifth lumbar vertebra, he caused congestion and rise of temperature without impairment of motion or sensation in the corresponding limb.<sup>1</sup> Did it, then, follow that the calibre of the vessels was regulated by the ganglia of the sympathetic, through fibres supplied by them to the various mixed nerve-trunks throughout the body? Were the sympathetic ganglia so many independent vaso-motor centres? By no means. It was found that by cutting the *rami communicantes* connecting the sympathetic ganglia with the spinal nerve-roots the same effect was produced upon the vessels as by division of the sympathetic trunk, or of the centrifugal fibres proceeding from it.

Accordingly it came to be admitted that the vaso-motor fibres were all derived, however indirectly, from the spinal cord. They might not make their exit from the vertebral canal by the same paths as the motor and sensory fibres destined for the same parts of the body. They might spring from a different region of the cord altogether, and pursue a relatively devious course through the sympathetic chain before setting out for their area of distribution. But in the last resort they might all be traced to the cerebro-spinal axis, the vast majority of them issuing from it with the anterior roots of the spinal nerves.

The conception of the vaso-motor system which flowed spontaneously from such facts as these was a very simple one. It may be concisely formulated in some such fashion as this. The muscular arteries throughout the body receive their nervous supply from the cerebro-spinal axis through the medium of those trunks and fibres to which the topographical anatomist assigns a certain independence under the name of the "sympathetic." The vaso-motor fibres are analogous in function to the musculo-motor nerves derived by the voluntary muscles from the same ultimate source. They maintain the

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<sup>1</sup> "Récherches Expérimentales sur les Nerfs Vasculaires et Calorifiques du grand sympathique." 'Journal de la Physiologie, tome v, 1862.

annular fibre-cells in a continual state of medium contraction ("arterial tonus"). When they are cut, the contractile elements are paralysed and allow the arterioles to be distended by the pressure of the blood from within. When they are stimulated, either electrically, chemically, or mechanically, they cause the fibre-cells to contract, and narrow the bore of the small arteries. Looked at in this way, the vaso-motor nerves are only capable of maintaining or increasing arterial contraction; they may, therefore, be termed *vaso-constrictor* nerves. Along these vaso-constrictor channels a perpetual stream of nervous energy is flowing from the cerebro-spinal axis, in order to maintain the normal tonus of the vessels.

The next step was to ascertain whether the centrifugal influence in question was derived from the cerebro-spinal axis as a whole, or from some special part of it. It was easy to show that vaso-motor fibres were given off from the cord with the anterior roots of all the spinal nerves; but this was only their "superficial" origin, to use an expression familiar to anatomists; their "deep" origin might be elsewhere—in the *medulla oblongata*, or in some part of the brain. The exact localisation of the vaso-motor centre was principally due to the labours of Owsjannikow, working under Ludwig's direction—though both Schiff and Von Bezold had previously pointed to the *medulla oblongata* as its seat. The animal employed by Owsjannikow was the rabbit.<sup>1</sup> The carotid artery was connected with a mercurial kymograph, and the mean arterial pressure determined. Through an opening made in the skull with a trephine, a thin narrow-bladed knife was introduced, and the brain was sliced transversely from before backwards, the effect of each transverse section on the blood-pressure being noted. The latter was found not to be appreciably affected so long as the line of incision lay in front of, or on a level with, the *corpora quadrigemina*. When the parts behind them were cut across, a marked fall of arterial pressure occurred, and continued to increase as the line of section was moved farther back. It was not enough, however, to determine the mere fact of a diminution of arterial pressure taking place. It was necessary to ascertain whether reflex contraction of the arterioles could still be excited by irritation of a sensory nerve. So long as this could be done, the hinder limit of the vaso-motor centre could not have been reached. It was found that when the *medulla oblongata* had been cut across some four or five millimètres in front of the point of the *calamus*, no rise of arterial pressure could be produced by galvanising the central stump of the

<sup>1</sup> "Die tonische und reflectorische Centren der Gefässnerven." 'Ludwig's Arbeiten,' 1871.



sciatic. Accordingly, it was concluded that the vaso-motor centre was situated between a point one millimètre behind the *corpora quadrigemina*, and a point from four to five millimètres in front of the apex of the *calamus*. The distance between these points in the rabbit amounts to four millimètres, and corresponds to a portion of the floor of the fourth ventricle. Owjannikow satisfied himself, moreover, that the vaso-motor centre did not occupy a median position, but consisted of two bilaterally symmetrical halves; for a deep longitudinal incision could be made in the middle line without lowering the arterial pressure, and without abolishing the reflex contractility of the arterioles.

The vaso-motor centre having thus been localised in the medulla oblongata, the maintenance of the arterial tonus is assigned to it as its proper function. We have just seen that any damage to this centre, or its separation from the vaso-motor nerves by division of the cord in the cervical region, is followed by relaxation of the small arteries all over the body, and a great fall of blood-pressure. Conversely, its excitation, by means of electricity, may be shown to cause general contraction of the arterioles, and a rise of blood-pressure. The constancy with which the tonus of the arteries is kept up implies a corresponding constancy of operation on the part of the vaso-motor centre. Is its unresting activity spontaneous, requiring only a supply of oxygenated blood for its performance? or does it depend on centripetal stimuli, perpetually conveyed to it along afferent fibres? The latter is probably the correct view. At any rate, we know that the calibre of the small arteries may be greatly altered, either in a plus or in a minus direction, by stimulating certain afferent nerves. Faradisation of the central end of a divided sensory nerve, such as the fifth, or of a mixed nerve, such as the sciatic, causes universal, or all but universal, contraction of the arterioles. On the other hand, we are acquainted with one very singular afferent nerve which exerts an influence of a directly opposite kind; when it is cut, and its proximal end excited, the small arteries throughout the body, and especially in the abdominal viscera, become relaxed. Owing to the great fall of blood-pressure which thus ensues, this curious nerve is known as the "depressor."<sup>1</sup> In the rabbit, its fibres pass from the heart through the inferior cervical ganglion to join the trunk of the vagus on a level with the thyroid cartilage; it may be isolated in the neck, where it lies on the carotid artery. The two kinds of afferent fibres, one of which, when excited, causes contraction, while the other causes relaxation

<sup>1</sup> Ludwig and E. Cyon, "Die Reflexe eines der sensiblen Nerven der Herzens auf die motorischen der Blutgefäße." 'Ludwig's Arbeiten,' 1866.

of the vessels, are supposed, on the usual theory, to act upon the vaso-motor centre, either exalting or depressing its functional activity.

So much for the simple and well-knit theory of vaso-motor action which is most in favour at the present time. It is taught, with more or less of limitation, burdened with a greater or smaller number of conditioning clauses, according as the teacher happens to prefer accuracy to clearness of exposition, or *vice versâ*. Upon its absolute or approximate fidelity to fact a number of prevalent doctrines, both in pathology and therapeutics, are made to rest. So far, we have not had occasion to speak of any other phenomena which are incapable of being reconciled with it. We shall now proceed to consider facts, some of which can only be brought into harmony with the theory in question by dint of a good deal of stretching and manipulation, while others escape altogether from its trammels. Some attempt will then be made to construct a provisional hypothesis, or framework of hypotheses, which may suffice, at any rate, to hold the facts together without any very glaring degree of incoherence,—which may bear the same relation to the complete theory, when that arrives, as a digest of the law bears to a completed code.

We have hitherto recognised only one order of vaso-motor nerves, proceeding from the vaso-motor centre to the muscular coat of the small arteries. When these nerves, of which the cervical sympathetic is the type, are cut across, the arterioles become relaxed and dilated; when they are stimulated, the arterioles contract. Now M. Bernard, when carrying on an inquiry into the innervation of the salivary glands, stumbled upon the curious fact that the secreting function of the submaxillary gland could be excited by stimulating the peripheral end of the divided lingual branch of the fifth; further, that this secretory activity was attended by great dilatation of the vessels distributed to the secreting tissue. The former of these points had been noticed previously; the latter was new. It was ascertained, moreover, at an early period, that the phenomena in question were due to the excitation, not of fibres proper to the lingual, but of filaments which it obtains by anastomosis from the chorda tympani branch of the facial. The submaxillary gland, as we now know, receives its nervous supply from two distinct sources—from the superior cervical ganglion of the sympathetic, and from the chorda tympani through the medium of the lingual. Both sets of fibres are in connection with the blood-vessels of the gland; but they exert a precisely opposite influence on their calibre. Stimulation of the sympathetic filaments causes constriction; stimulation of those derived from

the chorda tympani, dilatation of the vessels. It may be added that the dilatation consequent on electrification of the chorda tympani is greater in degree than that produced by section of the sympathetic fibres.

Here, then, we have a vaso-motor nerve endowed with vaso-dilator instead of vaso-constrictor properties. So anomalous a fact was not allowed to stand without attempts to reconcile it with the accepted view of vaso-motor action. It was suggested, for example, that the vascular dilatation might be a secondary phenomenon due to a *vis à fronte* exerted upon the blood by the secreting elements of the gland, thrown into a state of activity by direct nervous agency. The chorda tympani would thus have remained a secretory nerve, only capable of rousing the gland-cells to activity; and the vessels would have been left, as before, under the exclusive control of the filaments supplied to them from the sympathetic. Plausible as this view may seem at first sight, it has been completely discredited by the progress of research. It is now universally admitted that the chorda tympani contains two sets of fibres endowed with totally distinct properties; one set is in connection with the secreting elements, whose functional activity it is able to rouse; the other set is in connection with the vessels, on which it exerts a dilating influence. The experiments of Heidenhain and Keuchel on the action of atropia on the salivary secretion are of themselves sufficient to establish the truth of this view. They found—and their discovery has since been repeatedly confirmed—that the secretory fibres of the chorda tympani may be completely paralysed by the administration of atropia, while its vaso-dilator fibres remain intact.<sup>1</sup>

This was the first observed instance of vascular dilatation produced by the centrifugal propagation of excitement along a vaso-motor nerve. The phenomenon could not be attributed, like the reflex dilatation consequent on stimulation of the depressor nerve, to an inhibitory influence exerted on the vaso-motor centre. Whatever might be the nature of the influence, it was clearly independent of the cerebro-spinal axis altogether. And thus the existence of vaso-dilator as well as of vaso-constrictor fibres became an established fact. Search has been made since then for other vaso-dilator nerves, and it has not been made in vain. Vulpian, to whom this branch of the subject is greatly indebted, has pointed out that the vaso-dilator action of the chorda tympani is not confined to the vessels of the submaxillary gland, but extends to the anterior portion of the tongue likewise. Stimulation of the peripheral

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<sup>1</sup> Heidenhain in 'Pflüger's Archiv,' vol. v.

end of the divided nerve causes marked congestion of the mucous membrane in this region. Moreover, the same physiologist has lately discovered that vaso-dilator fibres are supplied to the base of the tongue by the glosso-pharyngeal nerve.<sup>1</sup> When it is divided in the dog, and its peripheral end galvanised, the mucous membrane from the base of the epiglottis to a line a little in front of the circumvallate papillæ becomes intensely red and congested on the same side of the body. This congestive reddening is likewise apparent on the anterior pillar of the fauces, on the tonsil, and the velum palati. It persists for some minutes after the electrical stimulation of the nerve has ceased; it then disappears entirely, but may be reproduced again and again by repeating the original stimulus. It is a fact of some importance that these vaso-dilator fibres are not derived by the glosso-pharyngeal from the portio dura. Their origin must, therefore, be quite distinct from that of the corresponding fibres contained in the chorda tympani.

There is yet another pair of nerves, which undoubtedly belong to the same category. These are the *nervi erigentes*, first studied and described by Eckhard.<sup>2</sup> They arise, in the dog, from the sacral plexus, and are usually two in number, one on each side. Passing forward, they enter the hypogastric plexus, beyond which it is impossible to trace them. There can be no doubt, however, that their terminal filaments are distributed to the *corpora cavernosa*. These structures likewise receive branches from the pudic nerves; neither section nor irritation of the peripheral stump of the latter appears to cause any changes of a marked or constant character in the penis. They serve only to transmit the centripetal stimuli by which the act of erection is normally provoked. The behaviour of the erector nerves is very different. They may be cut without any appreciable consequences; but when the peripheral end of one, and, still more, of both nerves, is stimulated by an interrupted current, the *corpora cavernosa* become turgid with blood. The erection is not complete, the turgescence being most marked in the bulb, least so in the glans penis. If an incision has been previously made into the *corpora cavernosa*, blood is seen to issue from it more rapidly and of a brighter colour when the nerves are excited.<sup>3</sup> Lovén has succeeded in showing

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<sup>1</sup> Vulpian, "De l'action vaso-dilatatrice exercée par le nerf glosso-pharyngien sur les vaisseaux de la membrane muqueuse de la base de la langue." 'Comptes Rendus,' 1 Février, 1875.

<sup>2</sup> Untersuchungen über die Erektion des Penis beim Hunde. 'Eckhard's Beiträge,' No. vii, 1863.

<sup>3</sup> "Ueber die Erweiterung von Arterien in Folge einer Nervenirregung." 'Ludwig's Arbeiten,' 1866.

that the pressure in the efferent veins is simultaneously raised. That these phenomena are really due to dilatation of the arterioles supplying the erectile tissue with blood, and not to any hindrance offered to the escape of blood by compression of the efferent veins, has been adequately proved both by Eckard and Lovén. They have shown—1st. That ligation of all the efferent veins is not enough, *per se*, to produce erection. 2nd. That partial erection may be caused by electrification of the *nervi erigentes*, even when the efferent veins have been freely opened. This must not, of course, be taken to prove that retarded efflux of blood from the penis has *no* share in the phenomenon of erection; on the contrary, there are many reasons for thinking that it plays an important, though subsidiary, part in the process.

We have now enumerated three sets of nerves whose normal function is to convey vaso-dilator impulses from centre to periphery. Their vaso-dilator properties are universally recognised, though variously interpreted. Do they stand alone, or are there any others like them? Similar properties have been attributed by Schiff to the auriculo-temporal nerve of the rabbit, by Bernard to branches of the vagus distributed to the kidney and to fibres contained in the mylo-hyoid nerve. Vulpien, with every desire to see the number of vaso-dilator nerves increased, cannot bring himself to admit the claims of any one of the three that have just been mentioned. We shall presently find, however, that his hesitation is not shared by all his brethren, and that Goltz, in particular, is disposed to extend the domain of vaso-dilator action with a boldness that threatens to revolutionise our whole conception of this department of physiology.

We have hitherto taken it for granted that the vascular relaxation following section of vaso-motor nerves is a purely paralytic phenomenon. The tonus of the arteries being maintained by the constant operation of the vaso-motor centre in the medulla through vaso-constrictor fibres, division of those fibres must cause paralysis of the contractile elements of the arterial wall by separating them from their normal centre; just as a voluntary muscle is paralysed by section of its motor nerve. The vaso-dilator phenomena described above are not in any way antagonistic to this view.

But the new doctrine, of which Professor Goltz of Strasbourg is the apostle, if not the founder, has for its object so to generalise the principle of vaso-dilator action as to make it all but co-extensive with the entire group of vaso-motor phenomena. The remarkable clearness and logical force by which his style is distinguished, render his papers interesting and

suggestive reading, even to those who, like the present writer, are not disposed to go along with him beyond a certain point. In a paper that he contributed to *Pföüger's Archiv*<sup>1</sup> in 1873, the two following propositions are laid down and supported by experimental evidence:—1st, the tonus of the arteries is maintained by local centres situated in their own immediate vicinity, and is wholly independent of the cerebro-spinal axis, and of the great chain of sympathetic ganglia; 2nd, the dilated condition of the vessels which occurs on section of a mixed nerve such as the sciatic, is really due, not, as usually believed, to paralysis of vaso-constrictor, but to irritation of vaso-dilator fibres. To the first of these propositions we shall be obliged to return subsequently; it will be enough, for the present, to state very briefly on what sort of evidence it is based. Goltz affirms that the abolition of arterial tonus in any vascular area, caused by division of its vaso-motor nerves, is never permanent; that the tonus is invariably restored, *after the lapse of a certain time*. It is to their neglect of this element of *time* that he ascribes the otherwise inexplicable silence maintained by physiologists in regard to this point. Let us now proceed to consider the second of the above propositions in a little more detail. The evidence in its support, contained in Goltz's first paper, was inadequate for proof though sufficient to arouse attention. He cut the sciatic of a dog as high up as he could reach it; the temperature of the corresponding paw rose, owing to dilatation of the cutaneous vessels. After some days had elapsed, the arteries returned to their normal calibre, and the temperature of the two hind paws was found once more to be nearly equal. The same nerve was then cut a second time, at a lower point. The phenomena of vascularisation and rise of temperature were again as well marked as on the first occasion. Now, on the usual theory, the effects of the first operation would have been ascribed to paralysis of the vaso-constrictor fibres contained in the sciatic trunk, owing to their separation from the vaso-motor centre; the restitution of arterial tonus, to the existence and vicarious activity of vaso-constrictor fibres conveyed along other channels than the sciatic (*e. g.*, in the outer coat of the femoral artery) and consequently escaping the knife; while the dilatation of the vessels following the second operation would have remained altogether inexplicable. The following is the explanation offered by Goltz. He assumes that the tonus of the arterioles is governed by local centres of a ganglionic nature, connected with the cerebro-spinal axis by vaso-dilator fibres running in the trunk of the sciatic. Section of the

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<sup>1</sup> "Ueber gefäßerweiternde Nerven," *Pföüger's 'Archiv.'* viii, p. 174.

nerve, by irritating these fibres, causes vascular dilatation which lasts until the irritant effect of the operation has passed off, when the local centres are able to resume their normal tonic activity. Renewed section of the nerve at a lower point, though obviously incapable of in any way affecting the connexion of the vessels with the vaso-motor centre, renews the irritation of the vaso-dilator fibres with the same consequences as before.

These experimental results were fully confirmed by Putzeys and Tarchanoff.<sup>1</sup> They give in an unqualified adhesion to the first of Goltz's two propositions,—that relating to the existence and activity of local vaso-motor centres,—but they refuse to accept the second. The facts on which it rests they willingly corroborate; only pointing out, what Goltz had failed to notice, that the dilatation of the vessels caused by irritation of the sciatic is frequently, if not invariably, preceded by a transient stage of contraction. Though giving up the idea that arterial tonus depends exclusively on the continuous operation of a vaso-motor centre in the medulla, they retain the usual doctrine that the fibres connecting the peripheral vaso-motor apparatus with the spinal cord are all of the vaso-constrictor kind. Accordingly they regard the vascular dilatation following section of the sciatic, as due to paralysis of vaso-constrictor, not to irritation of vaso-dilator fibres; in short, they hold fast to the theory founded by Bernard on his experiments on the cervical sympathetic of the rabbit. They endeavour to overcome the great difficulty presented by the results of double section of the sciatic, by setting up a hypothesis decidedly more strained and artificial than that propounded by Goltz. It has all the appearance of an attempt to reconcile the new observation with the old theory at any cost. Professor Goltz has since made a fresh contribution to the subject, maintaining his original thesis, and supporting it by a modification and amplification of his fundamental experiment, of which the following is a brief outline.<sup>2</sup>

A vigorous and well-nourished dog having been chosen, its spinal cord is exposed and divided on a level with the last rib; this operation is performed under chloroform, and its object is to render all subsequent interference with the hinder part of the body absolutely painless, and thus to do away with all necessity for the disturbing influence of anæsthetics. The operation is immediately followed by a considerable rise in the

<sup>1</sup> "Über den Einfluss des Nervensystems auf den Zustand der Gefäße," Reichert and Dubois-Reymond's 'Archiv.,' 1874, 3 and 4.

<sup>2</sup> "Zweite Abhandlung über gefässerweiternde Nerven," Pfüger's 'Archiv.,' xi, 52 (1875).

temperature of the hind limbs, which are seen to be markedly congested. After some days, the tonus of the vessels is found to be entirely, or in great part, restored, and the experiment enters on its second phase. The two sciatic nerves are exposed, and divided as high up as possible; they are then dissected out from their connexions and completely isolated as low down as the popliteal space. The long stumps are then replaced *in situ*, and the incisions closed with sutures. The immediate consequence of this manœuvre is a fresh rise of temperature in both hind limbs, which subsides like the previous one, in a variable number of days. After it has subsided the experiment enters upon its third and last stage. One of the wounds is re-opened, and the long stump of the nerve, much inflamed and swollen, is raised from its bed among the muscles. A thermometer is introduced between the toes of the corresponding paw, and the nerve is then partly snipped, partly bruised, with scissors, as frequently as possible, beginning at its free extremity and proceeding downwards to the popliteal space. An interval of ten seconds is allowed to elapse between each movement of the scissors, and the height of the mercurial column is noted at the same intervals. The temperature of the paw is found to rise steadily during the whole period of the operation; the cutaneous blood-vessels may be seen to become enormously dilated, and their dilatation does not appear to be preceded by any stage of contraction, however brief. The phenomena are not complicated, either by anæsthetics, or by consciousness of pain; the animal often going on with his food and paying no attention to what is being done to his hinder extremities. When the nerve has been crushed throughout its whole length, a difference of  $10^{\circ}$  C., may often be observed between the two hind paws; the temperature of the one operated on being only a little below that registered by a thermometer in the rectum.

This very striking experiment tells strongly in favour of both of the propositions advanced by Goltz. Putting aside for the moment the question of local vaso-motor autonomy, let us enquire whether the hypothesis put forward by Putzeys and Tarchanoff, in order to reconcile Goltz's first experiment with the usual theory of vaso-motor action, is in any way adequate to explain the modified form of that experiment which has just been described. They argued that the vaso-constrictor fibres contained in the sciatic were first of all paralysed (relaxed state of vessels following first section of the nerve); that the vicarious activity of the local centres was then supported by a "latent irritation" of the vaso-constrictor fibres in the inflamed stump of the sciatic (restitution of the vascular tonus); lastly, that the vascular relaxation following the division of the sciatic



for the second time, was due to removal of the influence exerted by the irritated portion of the stump upon its peripheral branches. This rather clumsy and improbable hypothesis is entirely demolished by the results of Goltz's modified experiment. The rise of temperature in the paw after repeated crushing of the nerve is very much greater than that which occurs when it is simply cut across a second time. The "latent irritation" of the stump, supposed to be capable of maintaining the activity of the vaso-constrictor fibres after their separation from the vaso-motor centre, may possibly be annulled by section of the nerve-trunk at a lower point, but cannot surely be impaired by repeated crushing. Moreover, if the results of the operation are due to irritation and not to paralysis of vaso-motor fibres, it ought to be possible to produce effects similar in kind, if not in degree, by applying other stimuli, mechanical, chemical, or electrical, to the distal end of the divided nerve. This Goltz has done. He has excited the sciatic stump by means of Heidenhain's tetano-motor or nerve-hammer, he has touched it with strong sulphuric acid. In both cases extreme vascular dilatation was produced without previous contraction. Stimulation by an interrupted current of electricity caused a slight and momentary contraction, followed by persistent and enormous dilatation.

Though we may not be disposed to follow Goltz so far as to regard the vascular relaxation consequent on division of nerves as invariably due to irritation of vaso-dilator, and never to paralysis of vaso-constrictor fibres, his experiments compel us to admit that fibres of the former kind are much more universally distributed to the arterioles than even Vulpian is inclined to believe.<sup>1</sup> The assumption that fibres of both kinds are supplied to all the cutaneous vessels, perhaps to the visceral and muscular vessels as well, may seem a rash one. Without it, however, we find ourselves driven to resort to explanatory hypotheses of a still more strained and improbable kind in order to explain the facts.<sup>2</sup> We have yet to consider the true nature of the influence exerted by the vaso-dilator nerves on the calibre of the arterioles; before doing so, however, it will be necessary to say a few words about the first of the two propositions laid down by Goltz, viz. that the tonus of the vessels is maintained

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<sup>1</sup> See Vulpian, '*Leçons sur l'Appareil Vaso-moteur*,' 1875, vol. ii, p. 467.

<sup>2</sup> Kendall and Luchsinger, who have lately repeated some of Goltz's experiments, adopt the view that all the local ganglionic centres, by which the tonus of the vessels is normally maintained, are connected with the cerebro-spinal axis both by vaso-constrictor and vaso-dilator fibres. Some such hypothesis, however unlikely, is indispensable. Pfüger's '*Archiv.*,' xiii, 4 and 5, 1876.

by local centres, and is essentially independent of the vaso-motor centre in the medulla oblongata.

The nature of the evidence on which the belief in local vaso-motor autonomy is based has already been alluded to. When any vascular area, whether in a mammal or a frog, has been completely isolated from its connexions with the cerebro-spinal axis and the great chain of sympathetic ganglia, its arterioles are found to be relaxed; but their contractile power is not by any means abolished; sooner or later, without any renewal of their central connexions, they return to that state of medium contraction which is known as their tonus. Though Goltz has done a great deal to promote this view, and has illustrated it with many arguments and observations, it would be a mistake to suppose that it is altogether novel. The facts had not escaped notice; they had been correctly interpreted. It is really surprising that the luminous researches of Professor Lister, "*On the Parts of the Nervous System Regulating the Contraction of the Arteries*," published in the '*Philosophical Transactions*' so long ago as 1858, should have exerted so small an influence on our teaching. It is only within the last few years that his conclusions have begun to leak into this country from Germany, where they have been independently arrived at. The evidence he brought forward to prove the existence of a local vaso-motor apparatus, was derived, almost exclusively, from observation of the partial contractions exhibited by the small arteries in the frog's web after complete removal of the cerebro-spinal axis. Two of his conclusions deserve to be shortly recapitulated; for, in the opinion of the present writer, they embody the pith of most of the recent observations on the subject. They are: 1. That there exists in the frog's limb some means, probably ganglionic, by virtue of which the fibre-cells of the circular coat of the arteries may contract in concert with each other, independently of the cerebro-spinal axis or of any ganglia contained in the trunk. 2. That the local co-ordinating apparatus, though capable of independent action under special conditions of direct irritation, is, under ordinary circumstances, in strict subordination to the spinal system; while a remarkable provision exists for the maintenance of this control, notwithstanding *almost* complete severance of nervous connexion between the cord and the limb. All this is as true now as on the day it was written; but the seed appears not to have fallen on a congenial soil.

We know very little about the anatomical structure and position of these local vaso-motor centres, though we may be sure that they exist in close proximity to the walls of the vessels. That they consist of ganglionic corpuscles, either solitary or

agminated, may fairly be inferred from what we know of their functions. In forming an idea of their nature we may obtain some aid from the analogy of those larger, though still very minute, ganglia, which have been demonstrated in connection with such nerves as the chorda tympani, the glosso-pharyngeal, the erectors of the penis; in the walls of the bladder, the intestines, and the heart. Several histologists, among whom Dr. Beale occupies the foremost place, have asserted that many corpuscles and fibres in close relation to the minuter blood-vessels, formerly regarded as connective-tissue corpuscles with their stellate processes, are really nervous organs connected with a network of extremely delicate fibres ramifying upon the contractile fibre-cells. It is highly probable that these may be the centres with which we are now concerned.

The normal business of these perivascular centres is to keep the arterioles in a partially contracted state. They are essentially vaso-constrictor centres. Any exaltation of their activity is followed by vascular contraction; when their activity is depressed or inhibited, the vessels become relaxed. Accordingly we must inquire what are the channels along which either excitant or inhibitory impulses are conveyed to them. There is great reason to believe that these channels are of two kinds, one set transmitting impulses from the skin or tissues, the other from a vaso-motor centre or centres higher in the scale of generality. Moreover, each of these two sets must consist of fibres belonging to opposite or antagonistic categories, the one excitant, the other depressant or inhibitory; unless we are disposed to assume that one and the same fibre may convey both kinds of impulses, according as the stimulus applied to it is of high or low intensity. The functional activity of the perivascular centres will thus be regulated, partly by centripetal impulses derived from the skin and tissues, partly by impulses discharged in a centrifugal direction from higher centres.

By way of illustrating the above statements, let us consider for a moment the vascular changes produced in the web of the frog's foot by the local application of an irritant. These changes have been thoroughly investigated by Lister.<sup>1</sup> The first effect that ensues upon the application of an irritant to a very limited portion of the web is contraction of the small vessels; but this is momentary, and is followed by dilatation. Neither the anæmia nor the active congestion produced in this way are coextensive with the irritated area; they cannot, therefore, be ascribed to any immediate action of the irritant upon the tissue-elements, but must require the agency of the nervous

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<sup>1</sup> "On the Early Stages of Inflammation," *Phil. Trans.*, 1858.

system for their production. They are reflex phenomena. Now, it was shown long ago by H. Weber<sup>1</sup> that they could be produced after the web had been completely isolated from the nervous centres contained in the trunk of the body. The reflex circuit consists, in all likelihood, of centripetal fibres distributed to the skin, local perivascular centres, and centrifugal fibres connecting these centres with the annular fibre-cells. The primary contraction is due to stimulation of the centres, the secondary dilatation to their activity being depressed. It is not very clear whether we ought to assume two kinds of centripetal fibres, "pressor" and "depressor;" the facts are compatible with the view that the fibres are of one kind, the varying results being due to varying intensity of stimulus.

When the arterioles are made to contract or dilate by centrifugal impulses conveyed from the cerebro-spinal axis, it is still to modifications in the state of the local perivascular centres that the changes of arterial calibre are immediately due. Stimulation of vaso-constrictor fibres exalts the normal activity of those centres; irritation of vaso-dilator fibres depresses it. Indeed, it would be far from easy to conceive the mode of operation of so-called vaso-dilator impulses without admitting the existence of a local vaso-motor apparatus. If we reflect on the anatomical arrangement of the contractile elements of the arterial wall we see at once that anything like active dilatation is impossible. The fibre-cells are disposed at right angles to the axis of the vessel. When thrown into contraction they can only diminish its calibre. When paralysed or relaxed they permit the vessel to become distended by the pressure of the blood from within. The dilatation is thus an entirely passive phenomenon, so far as the muscular coat of the vessels is concerned. Accordingly, vaso-dilator impulses can only operate by inhibiting the normal activity of the local centres; and "vaso-dilator" fibres may, with more approach to accuracy, be termed "vaso-inhibitory." By substituting the latter epithet for the former one, moreover, the analogy which undoubtedly exists between the innervation of the heart and that of the contractile blood-vessels is brought into greater prominence. This analogy is strongly insisted on by Goltz, and for purposes of illustration it is undoubtedly of considerable value. Just as the normal contractions of the heart are kept up by the continuous operation of the intra-cardiac ganglia, so the tonus of the arteries is maintained by perivascular centres; just as the activity of the intra-cardiac ganglia may be augmented by impulses conveyed along the accelerator nerves, so that of the

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<sup>1</sup> Vulpian, op. cit., i, 171, ii, 472.

perivascular centres may be stimulated through the vaso-constrictor fibres in connection with them; lastly, the inhibitory function of the vagi affords an exact parallel to that of the vasodilator or vaso-inhibitory nerves. The analogy between the heart and the arterioles must not, however, be pushed too far. For instance, the contractions exhibited by the arterioles in the frog's web, after complete separation of the limb from the higher nervous centres, resemble the vermicular movements of the intestines more nearly than the rhythmical pulsations of the excised heart. Rhythmical movements have, indeed, been observed in various arteries, *e.g.* in the main artery of the rabbit's ear by Schiff, in the frog's web by Saviotti, in the frog's mesentery and the saphenous artery of the rabbit by Riegel; such movements are, doubtless, dependent on local centres; but our knowledge concerning them is too incomplete to enable us to grasp their true meaning. Lister is probably right in thinking that the spontaneous movements of the small arteries are more nearly analogous to those of the intestines than to those of the heart. He has shown that the contractions of the unstriated muscular fibres in the coats of the bowel, which result from arrest of the circulation, are not due to any influence exerted directly upon the contractile tissue, but that the intestinal nerves are essential to their production. "Thus," he says, "we have support from analogy for the view that the muscular contractions which occur under similar circumstances in the arteries are induced by nervous agency." The plexuses of Meissner and Auerbach may be taken to represent the perivascular centres; the fibres distributed to those plexuses may, perhaps, be viewed as analogues of the vaso-constrictor and vaso-inhibitory fibres which terminate in the centres in question; and there is every reason to believe that the intestinal movements are susceptible of being augmented or inhibited by a reflex mechanism more or less similar to that by which reflex congestion or anæmia are induced.

The vaso-motor centre in the medulla oblongata has now been robbed of its prerogative as the *sole* regulator and maintainer of arterial tonus; still, it is able to influence the calibre of the small vessels indirectly through their local centres. Are we to conclude that even in this capacity it stands alone? Are the vaso-constrictor and vaso-inhibitory impulses transmitted to the perivascular apparatus from the higher centres derived exclusively from the medulla? By no means. If we are to bring the experimental data furnished by recent research into anything like harmonious order we must assume a whole series of vaso-motor centres, standing to one another in a hierarchical relation of progressively increasing generality of operation.

Next above the perivascular centres are the sympathetic ganglia connected either with the various plexuses in the great cavities of the body, or forming part of the prevertebral chain. Above them in the scale are a series of vaso-motor centres *échelonnés* down the whole length of the cord. Higher still is the vaso-motor centre in the floor of the fourth ventricle. Lastly, various parts of the brain, including the grey matter of the cerebral convolutions, are capable of exerting an influence on the contractility of the arterioles.

It may be well to illustrate the above statements by a few examples calculated to give the reader an idea of the kind, though not, of course, of the amount of evidence on which they rest. First, then, as regards the sympathetic ganglia. The question of their functional autonomy has long been under discussion. With such names as those of Prochaska, Willis, and Bichat, on one side, while those of Meckel, Legallois, and Scarpa are arrayed on the other, the balance of authority may seem to be in equilibrium. If we address ourselves, however, to the narrower question, whether reflex vaso-motor phenomena can be produced through the sole medium of the sympathetic centres, we shall find ourselves constrained to answer it in the affirmative. Bernard has shown, for instance, that the secretory and vaso-dilator (or, more correctly, vaso-inhibitory) fibres of the *chorda tympani* are susceptible of reflex excitation after the nervous circuit concerned has been entirely separated from the cerebro-spinal axis. He cut the branches supplied to the submaxillary ganglion by the sympathetic; he then divided the lingual nerve *above* the point at which the filaments derived from the *chorda* separate themselves from it. Under these conditions, faradisation of the lingual nerve close to the tongue, was still followed by a flow of saliva from Wharton's duct, and by active congestion of the gland. The centripetal stimulus was conveyed by the lingual to the submaxillary ganglion; and the latter had power to excite the vascular and secretory fibres passing from it to the gland. Again, Vulpian has found that extirpation of the cervical ganglion of the sympathetic in the frog, is followed by congestion, not only of the corresponding half of the tongue, but of the entire buccal cavity on the same side. Now, this result may be obtained equally well after the cerebro-spinal axis has been destroyed, proving that the ganglion is an independent vaso-motor centre for the vascular territory in question.

The existence of independent vaso-motor centres in the spinal cord, below the medulla oblongata, is proved by the possibility of exciting reflex congestion and anæmia in the posterior half of the body after the influence of the medulla has been eliminated by

section of the cord. Vulpian has shewn that faradisation of the central end of the divided sciatic is followed by contraction of the vessels in the hind paw of the opposite side, even after the continuity of the spinal cord has been interrupted in the upper dorsal region. The centre, in this reflex circuit, is doubtless situated in the lumbar enlargement. So, too, Goltz has proved that erection may be produced by exciting the pudendal nerves after the spinal cord has been divided; in other words, that the centrifugal vaso-inhibitory fibres contained in the *nervi erigentes* have their centre in the posterior segment of the cord. There is abundant evidence of the same kind in support of the view that vaso-motor centres for different parts of the trunk are placed at intervals all down the spinal marrow.

The vaso-motor functions of the medulla oblongata have already been discussed; any further consideration of them would, therefore, be superfluous in this place. We may pass on at once to say a few words concerning the relation of the brain to the vaso-motor system. The paleness of terror, the flush of shame or of happiness, are of themselves enough to shew that Owjannikow's statements concerning the absence of vaso-motor centres in the brain proper must not be taken too literally. Moreover, direct experimental evidence has recently been brought forward to show that such centres, related to definite vascular areas in the trunk and limbs, are actually disseminated through the cerebral cortex. Eulenburg and Landois<sup>1</sup> found that destruction of certain limited portions of the cortical substance in one hemisphere was followed by relaxation of the arterioles in the limbs of the opposite side of the body. This vascularisation caused a rise of temperature amounting, in some cases, to 5°—7°, in others to 1½°—2° C. only, and lasting from one day to several weeks. Stimulation of the same cortical areas by induced currents was followed by a slight and transient fall of temperature in the opposite extremities (·2° to ·6° C.). The associated musculo-motor disturbances showed that these thermic or vaso-motor centres were situated in close proximity to the corresponding psycho-motor centres for the limbs. A successful attempt was made to differentiate the vaso-motor centre of the fore from that of the hind leg; the former was found to lie a little in front and to the outer side of the latter. Brown-Séquard<sup>2</sup> had previously observed that thermal irritation or mechanical injury of the cerebral surface might be followed by most of the effects which usually result from section of the cervical sympathetic, viz.

<sup>1</sup> 'Centralblatt für die Medizin. Wissenschaften,' April, 1876.

<sup>2</sup> 'Archives de Physiologie,' Octobre—Décembre, 1875.

afflux of blood and rise of temperature on the side corresponding to the injury, partial closure of the eyelids, contraction of the pupil. The degree of vascular paralysis caused by superficial cauterisation of the brain was noticed to vary with the extent of surface damaged. After discussing the possibility of the phenomenon being of reflex nature, due to irritation of trigeminal fibres present in the pia mater, M. Brown-Séquard concludes that the grey matter of the convolutions themselves is directly concerned in its production.

It may be said, broadly, that the facts in regard to vaso-motor action which have been discovered up to the present time do not admit of being understood or reconciled with one another, unless we adopt the view that vaso-motor centres, more or less capable of independent action, are disseminated throughout the whole of the nervous system. The activity of any one of these centres may be exalted or depressed by impulses transmitted from the surface of the body or the internal viscera through afferent fibres; also by impulses derived, through commissural or inter-central fibres, from centres higher in the scale. A vaso-constrictor or vaso-inhibitory impulse arriving at any centre, whether from one or from the other source, is forthwith reflected along vaso-constrictor or vaso-inhibitory fibres to the perivascular ganglionic apparatus, by the exaltation or depression of whose activity the ultimate contraction or relaxation of the arterioles is accomplished.

There are still a great number of phenomena belonging chiefly to reflex vaso-motor action, whose mechanism, even on this complicated theory, is difficult of explanation. The vascular contraction or dilatation following irritation of centripetal vaso-motor fibres may be limited to a particular area, or it may extend to all the contractile vessels in the body at once. Irritation of the central end of the divided dorsal nerve of the foot in the rabbit, for example, is followed by contraction of the arterioles all over the body, while the saphenous artery of the same limb becomes dilated. Should we be justified in concluding from this observation that the afferent nerve in question, "in common probably with other sensory nerves, contains fibres so endowed that, when they are excited, the action of the vaso-motor centre is inhibited or suspended, as regards certain regions with which the nerves in question are in close anatomical relation?"<sup>1</sup> Hardly so. It has been shown by Owsjannikow and Tschiriew,<sup>2</sup> for instance, that the general vascular contraction and rise of arterial pressure caused by

<sup>1</sup> Sanderson, 'Handbook for the Physiological Laboratory,' p. 246.

<sup>2</sup> Quoted by Vulpien, *op. cit.*, vol. i, p. 242.



stimulation of the central stump of the divided sciatic, are associated with a simultaneous relaxation of the vessels of both ears, and that this relaxation is more marked in the ear of the corresponding than in that of the opposite side. Here, then, is an instance of general vascular contraction associated with local relaxation; the area of relaxation standing in no recognised anatomical relation to the afferent nerve, by irritating which these anomalous results are attained.

But an enumeration of the difficulties which even the most recent developments of the vaso-motor theory are powerless to explain would lead us too far. Enough has been said to shew: first, that the simple theory of vaso-motor action laid down in even the more modern of our text-books is utterly inadequate to explain many of the phenomena that have been observed; secondly, that the vast majority of those phenomena may be co-ordinated by the aid of a hypothesis of a much more complicated kind, which we have done our best to explain; lastly, that we must expect the progress of enquiry to bring an ever increasing number of facts to light which cannot be subsumed even under this more complex theory.

What is the obvious lesson to be derived from these considerations? That we cannot be too careful in applying the working hypotheses of the physiologist concerning the vaso-motor system to the explanation of pathological and clinical facts, or of the action of remedies. For an excellent *résumé* of many such applications, which enjoy so great a popularity at the present time, the reader is referred to the second volume of M. Vulpian's 'Lectures on the Vaso-motor Apparatus.' This is perhaps a fitting place in which to express our obligations to that physiologist for the vast amount of conscientious labour that he must have bestowed upon their composition. The results of much patient study of work done by others, of original research, of acute critical discrimination, are clothed in language at once lucid and concise. The work may fairly be commended even to those who have but little leisure to devote to scientific literature.

For the manifold crudities which are every day put forth with the object of bringing the results of experimental research into harmony with those derived from the wards and the post-mortem room, the physiologist is not responsible. He may fairly say with Stoerck: "*non hypotheses condo, non opiniones vendito: quod vidi, scripsi.*" It is commonly the physician who, forgetting the caution which is perhaps the most scientific quality of the scientific mind, handles the provisional hypotheses which are indispensable for holding the vast accumulation of experimental facts together, as though they were laws

of nature, firmly established and abundantly verified. The student of physiology is well aware that his speculations about the vaso-motor system stand in much the same relation to the truth, as that in which the cycles and epicycles of the Ptolemaic astronomy stand to the astronomy of our own day. The Copernicus, the Kepler, the Newton of the nervous system are yet to come; and while we look for their coming, the wisest course is to abstain from hasty endeavours to explain the inexplicable, and to accept provisional hypotheses with thankfulness for what they are.

## II.—Altitude and Disease.<sup>1</sup>

THE handsome work of M. Jourdanet is very unlike other books. It is a half medical, half popular treatise in two large volumes, and has appeared in the form of what the French call an *édition de luxe*. It is full of maps and engravings. The former illustrate chiefly the altitude of various portions of the earth's surface, and are useful and instructive. Of the latter a portion are excellent. Most of the views of places are good, and we have no fault to find with the portraits of men of science and of mountain travellers. But when we look at a certain number of the illustrations which we are told were prepared "after the indications of the author," we cannot but think that they are entirely thrown away. Some of these views represent the dispersion of mankind from the Tower of Babel, the death-bed of the traveller Jacquemont, the fainting of Humboldt in ascending a mountain, and a view of a warrior descending into the sulphury mouth of a volcano.

The nature of the text is a good deal in keeping. A great deal of useful matter has been brought together, including accounts of balloon ascents and ascents of mountains. There is a large amount of information about the high lands of Asia, of Africa, and more particularly about those of Mexico and Peru. M. Jourdanet is most at home in his account of Mexico, and has reproduced the results of his long residence in that country. There is a full account of M. Bert's experiments with compressed

<sup>1</sup> 1. *Influence de la Pression de l'Air sur la vie de l'Homme. Climats d'Altitude et Climats de Montagne.* Par le Docteur D. JOURDANET. Paris, 1875, two vols., imp. 8vo, pp. 800.

2. *Les Climats de Montagnes considérés au Point de Vue Médical.* Par le Docteur H. C. LOMBARD. Genève, 1873, 12mo, pp. 232.

air. There are useful tables of heights in all parts of the world. In short, the work is brimful of information bearing more or less on the subject treated of; but there is diffuseness and want of method. We have various unnecessary descriptions, such as a dissertation meant to show that the atmospheric pressure was at one time much greater than it is now, and that this cause necessitated residence in the Asiatic mountains to early man. We have also a chapter that was not at all required respecting the illness and last moments of Jacquemont, devoted mainly to showing that he contracted the abscess of liver of which he died, during his journeys in the Himalayas. However, with these drawbacks, Jourdanet's work is still well worth studying.

Lombard's book is a very different one. It is compact and precise; it too is the result of many years' study of the subject, and Lombard has a local acquaintance with the Alps and Switzerland, which appears to us to be superior in extent, and probably in accuracy, to Jourdanet's knowledge of Mexico. Both of them seem to take up theoretical opinions perhaps too strongly, and Lombard's views regarding climatic zones in the Alps require modification. One idea runs all through Jourdanet's book,—that those who live at great heights are anæmic, while Lombard considers that their blood is imperfectly oxygenated, that there is a great tendency to emphysema, and that it is the antagonism of emphysema to phthisis that makes mountain climates useful in that disease. We shall not follow our authors into such questions, or other matters of theory, but shall glean from them and from others what is known of the effects of diminished and of increased atmospheric pressure on man, in health and disease; what are the diseases prevalent at considerable elevations; and from what diseases, if from any, those elevations enjoy an immunity.

Perhaps the following account will be made more intelligible by premising, that it may be said in a rough way, that there is a loss of atmospheric pressure according to elevation in about the following proportions:

Height of	2500 feet	.	.	loss of $\frac{1}{4}$	atmospheric pressure.
"	5000 "	.	.	" $\frac{1}{2}$	"
"	7500 "	.	.	" $\frac{3}{4}$	"
"	16000 "	.	.	" 1	"

Although in reality it is impossible to separate other elements, such as those of temperature and of moisture, we shall deal chiefly with the effects most dependent on pressure.

The influence of altered atmospheric pressure may be studied according as that pressure is increased or diminished. Atmospheric pressure is diminished by ascending heights, by rising

in the air in balloons, and its diminution may be artificially produced by the abstraction of a certain quantity of air from a perfectly closed vessel or chamber.

Glaisher's ascents in balloons are among the most remarkable, and have supplied much valuable information. He made observations on the pulse and on respiration. On the 17th of June, 1862, he found that his own pulse was 76, that of his companion 74, before starting. At the height of 16,000 feet his pulse had risen to 100, that of his companion to 84. After they had descended the pulse of both was 76. At the height of 18,000 to 19,000 feet both had violent beating of the heart with some difficulty of breathing, and after a short time their hands and lips became of a dark blue colour; at still greater heights the respiration was more impeded.

On the 18th of August both gentlemen had a pulse of 76 at starting; at near 20,000 feet the pulse became 98 and 100, and at greater heights 107 and 110; at 21,000 feet their face and hands became blue.

The greatest height was reached on the 5th of September not without serious distress. At 27,000 feet Glaisher became unconscious and his companion lost the power of using his hands, and had to open a valve with his teeth. Glaisher's fainting was heralded at 27,000 feet, by his not being able to see distinctly, and then by his not being able to move his arms or legs, although he could his neck. Next he lost his sight, although he could still hear; but when his companion called to him, he found himself unable to answer. He then became unconscious, and did not regain his consciousness until the balloon had descended to 27,000 feet. Glaisher had not moved in the balloon, while his companion had been obliged to move to manage the steering. Glaisher and his companion occasionally suffered from nausea at considerable elevations; thus Glaisher on the 17th of July, after some difficulty of breathing, had at between 23,000 to 25,000 feet all the feelings of sea-sickness. In the case of Gay Lussac and Biot in the year 1804, at the height of 12,000 feet the pulse of the former had risen from 62 to 80, that of the latter from 79 to 111.

The effects of ascending great heights have been described by very many authors, as Humboldt, Saussure, Boussingault, Schlagentweit, and others, and with especial accuracy of late years by Dr. Lortet, of Lyons. They may be thus summed up:—Quickened and laborious respiration, which, if exertion is made, may amount to dyspnoea; quickening of the beat of the pulse and heart; bleeding from the mucous membranes of the head; violent headache; sleepiness; loss of senses and of memory; mental depression; thirst; nausea; vomiting; loss of

power in the limbs. All these symptoms remit on rest, but return on renewed exertion.

All are not affected alike from ascending mountains. The amount of suffering varies in the same person at different times, and, strange to say, in some mountains it commences at a smaller elevation than in others. Those who make most exertion suffer most. Those who are on horseback suffer less than those on foot. The lower animals are affected as much as man.

The following were the experiences of Lortet in ascending Mont Blanc. They slept at the Grands Mulets, a height of 9500 feet, and no one felt any inconvenience; but as they ascended, the disagreeable effects gradually came on and increased up to the Plateau, when drowsiness and pain in the back of the head came on. Up to the Grand Plateau, 12,000 feet, there was no difficulty of respiration in those who were accustomed to mountain climbing. When he remained quiet, Lortet observed his respirations to be 24 in the minute, as at Lyons, but a little farther up he found them increased to 36, and even when he was at rest his breathing was short and laborious, with a feeling of constriction of the chest, and at the top the slightest movement caused want of breath. The difficulty of breathing gradually lessened after two hours' rest, and the respirations fell to 25, but were to a certain degree laborious.

It was ascertained by means of an apanograph that as they ascended, the quantity of air inspired diminished, and that the period of inspiration was shortened, while that of expiration was protracted. The inspiration did not pass with a pause and gradually into the expiration, but changed sharply into it.

Lortet's pulse at Chamounix before starting was 64, and it gradually rose as he ascended to 80, 108, 116, 128, and 136. The pulse was easily compressible. Above the height of 13,800 feet the veins of the hands, of the forearms, and of the temples became distended with blood, the colour of the face pale and slightly cyanotic. All, even the guide, felt at this height some swimming of the head and drowsiness. After two hours' rest on the summit the pulse was 90 to 108.

Soon after getting to the peak Lortet felt sick, and his pulse became thready; after an hour the nausea was gone, but the arterial tension of the pulse was feeble, and there was marked dirotism. It was two days after his return to Chamounix before his pulse became regular again. Nearly similar results in conditions of pulse were observed in an experienced guide and in Professor Chauveau, who accompanied Lortet.

Some of the phenomena described, such as the quick small

pulse, the beating of the heart, the feeling of lassitude, and even the nausea, might be occasioned by the exertion of climbing; but they did not occur until the height of nearly 10,000 feet had been reached. The progressive difficulty of inspiration and shortness of the expiration, even when they were at rest, could not be so explained, any more than the distension of the superficial vessels and the emptiness of the arteries. Lortet also observed increasing coldness of the body, which, as in the case of balloon ascents, cannot be entirely accounted for by the cold of higher elevations, but points distinctly to deficient oxygenation of the blood as its cause. This conclusion is enforced by the results of M. Bert's investigations, which appear to show that respiration does not depend on atmospheric pressure simply *per se*, but as it affords a greater or smaller supply of oxygen to the lungs. Acting on this principle, supplies of oxygen have of late been carried in balloon ascents, and in crossing some of the high passes of the Himalayas, chlorate of potash has been taken, on the theoretical idea that it will supply oxygen to the system; its use has been distinctly advantageous, if we are to accept the experience of Mr. Bellew and other travellers in the Himalayas, but we must remember that similar relief is afforded in the Andes by the use of the coca leaf.

Results like those, that have been detailed above, were produced by Vivenot with rarefied air in pneumatic apparatuses. He rarefied the air in two sittings to degrees corresponding to 11,000 and 14,000 feet of altitude. He made experiments on four people, and found in all an increase in the number of inspirations, and a quickening of the pulse of from two to twenty-five beats, with diminished arterial tension. There was also marked injection of the conjunctivæ, with burning of the eyes, a feeling of warmth in the skin of the face, some swimming of the head, and neuralgic pains in the forehead, all which symptoms disappeared when the atmospheric pressure was made normal again.

Bert has made experiments on the effect of rarefied air on the lower animals. He found that if the atmospheric pressure was taken off, they could not make use of the oxygen of the rarefied air, and therefore died. They ceased to be able to use it, when its density was five times less than that of air at the sea-level.

Taking all the symptoms developed under these different conditions together, there can be little doubt that the relative congestion of the venous and emptiness of the arterial system are caused by impeded circulation in the lungs. The difficulty of inspiration is thought by Dr. Liebig (who has studied the subject for many years) to result partly from the more active elastic contraction of the lungs owing to the diminished resist-

ance of the air, which the muscles of the thorax can scarcely overcome.

Thus are explained the acceleration of the pulse, the difficulty of respiration, the beating of the heart caused even by slight exertion at heights, and all the phenomena of headache, sleepiness, and lassitude, going on to fainting, as illustrated in the case of Glaisher in a balloon, or of Humboldt on a mountain. There is found in the comparative arterial emptiness of the capillaries of the brain an explanation of the nausea which may occur from diminished atmospheric pressure, or from over-exertion in climbing,

We must next shortly consider the effects of increased atmospheric pressure. They can only be studied under artificial circumstances, as in chambers prepared for compressed air, and in divers and others who work under increased atmospheric pressure; they cannot be very satisfactorily studied at the bottom of mines, as they seldom are in reality much below the sea-level. These effects may be thus summed up:—a diminished number of inspirations, which are increased in depth, and along with this a diminished frequency of the pulse, a diminution of diastole and subsidence of the wave of the pulse, higher tension of the arteries and relative fulness of them, and a more equable flow of the blood to the left heart. The circulation in the venous system is relieved, and there is a feeling of comfort instead of the feeling of depression which accompanies the extreme diminution of atmospheric pressure. There appears to be a temporary activity of molecular change, which is probably caused by the increased supply of oxygen.

Labourers working at the foundations of bridges under the pressure of two atmospheres not only feel no inconvenience during eight hours' labour, but feel less tired than when working under ordinary pressure, and when they work hard, they distinctly have their breathing less affected. There is an increase of muscular power instead of a loss of it, as observed under diminished pressure; men ascend with facility flights of steps under increased, which they could only do laboriously under ordinary atmospheric pressure.

But various unpleasant circumstances have been found to attend the transition from increased to natural atmospheric pressure. They have been observed both in men and in the lower animals. So marked have these effects been that they have been described by various authors, French and American, and Dr. Andrew H. Smith published a treatise on it<sup>1</sup> under the title of the Caisson disease.

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<sup>1</sup> Brooklyn, 1873.

These effects occur most readily in men who are unaccustomed to such changes of pressure, and a certain number of men, including full-blooded ones, may be considered to be predisposed to suffer from them.

When the change is too sudden, the workman may get bleeding of the nose, but much more serious consequences often arise, especially paralysis, and this mainly of the lower extremities. Sometimes the results have been fatal. The greater the atmospheric pressure the greater is the risk of this accident on its sudden removal, but it has happened to persons who had been working under not more than the pressure of two atmospheres. The paralysis usually comes on at once, but it occasionally occurs some minutes or even some hours after quitting the increased pressure. Similar phenomena have been observed by M. Bert in the lower animals, in fish, in dogs, and in cats. We mention cats in particular, as they are considered to be of all animals the ones that suffer most from the rarity of the atmosphere at mountain heights. In those animals, after they had suffered from convulsions and paralysis, the dorso-lumbar portion of the spine was found of a creamy consistence, without a trace of sanguineous exudation. M. Bert attributes those disagreeable effects to the formation of bubbles of gas in the blood. These are due to the nitrogen, which was dissolved in the blood during the continuance of the pressure, returning to the gaseous condition when the pressure is removed. The bubbles of gas arrest the circulation in various parts of the body, but especially in the lumbar portion of the spinal cord, where they give rise to softening and paraplegia. When they occur in large quantity, they obstruct the pulmonary circulation, distend the heart, and cause death more rapidly.

Dr. Smith is not inclined to accept M. Bert's explanation, as in cases where paraplegia has come on some hours after removal from great atmospheric pressure, the nitrogen must have escaped from the blood, and because he does not see how habit can give any immunity from the formation of bubbles of nitrogen, although it certainly does from the risk of such seizures. Smith attributes these accidents to congestion of the spinal cord and brain inside their unyielding bony coverings.

However this may be, all are agreed that the best mode of treating such accidents is, in the first instance, to restore the sufferer to increased atmospheric pressure, and that the only way of securing against these accidents is to remove the atmospheric pressure very gradually. But the treatment most readily available is the exhibition of stimulants.

We may state, in passing, that increased atmospheric pressure is sometimes used as a curative agent, and is said to mitigate



asthma and bronchial irritation; while, on the other hand, Jourdanet states he has relieved cerebral anæmia by the use of diminished pressure. But as there are inconveniences in applying rarefied as well as condensed air, the latter suggests that the abstraction or the addition of oxygen to the air inspired would be more convenient and equally efficacious.

Such is a short outline of what is ascertained about the effects of considerable changes of atmospheric pressure, viewing them as much as possible apart from other influences. We cannot, however, leave this part of the subject without pointing out that the system has a wonderful power of gradually accommodating itself to great alterations of pressure, as exemplified in those who learn to work under the surface at increased pressure, and in those who go to reside at mountain heights.

But though it is comparatively easy to define the effects of diminished and of increased atmospheric pressure when they are extreme, it is by no means easy to define the effects of slighter degrees of pressure. On the majority of people the changes indicated by the barometer, which in a year, or even a month, may be equivalent to ascents and descents of about 1500 feet, have no very marked effect, though they have on many sensitive people. Recent authors seem to have been able to connect various nervous seizures, and even hæmorrhage from the lungs and some sudden deaths in heart diseases with a rapid fall of the barometer.

Every individual who ascends from the level of the sea to a considerable height, is subjected to various alterations besides changes of atmospheric pressure. That of change of temperature is a very important one, and manifests its operation in the production of increased appetite, a diminution of the cutaneous and increase of the renal secretion, attended very often with inaction of the liver. The effects of mere rarefaction of the air are only distinctly appreciable at such heights as about 5000 feet in Europe and 7000 or 8000 in the Himalayas. It would seem probable that as mountain air is rarefied, less oxygen must be inhaled at each inspiration, unless the lungs take on an increased action, and that if the supply of oxygen is deficient, there must be more frequent respiration or an expansion of the lung-cells to make up for the deficiency. Coindet at Mexico found that at a height of 7410 feet, while the respiration was slightly more frequent, there was no alteration in the relation between circulation and respiration. He found the absolute quantity of carbonic acid expired to be practically the same as at the level of the sea,<sup>1</sup> and this is supported by the analogy of

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<sup>1</sup> He thought that new arrivals inspired less air and expired less carbonic acid

Frankland's experiment, which showed that the amount of combustion of a candle was almost the same on the summit of Mont Blanc as at its base. Others, however, chief of them Jourdanet, who is followed by Lombard, believe that there is diminished expiration of carbonic acid, that there is a deficiency of what he calls respiratory diet, and that the blood becomes charged with an excess of carbon. Armieux, in the case of 86 invalids removed from the plains to Barèges, at a height of 4000 feet, satisfied himself that after a residence of four months the respirations were increased by two, and the beats of the pulse reduced by four. He also found, on careful examination, that the 86 men had in the four months gained on an average one inch in girth round the chest. Dr. Kellett found that the invalids at Landour gained one inch chiefly during the first two weeks. On the whole it seems probable that on first going to the mountains the inspirations are more frequent but shorter, and therefore not so deep. Men going to stations in the Himalayas at 7500 feet complain at first of short-windedness, but it does not last; the pulse also is quickened, and has less force; and there is probably increased elasticity of the lungs. But on all these subjects we want a great deal of careful and continued observation; many of the facts of the case are still disputed, and granting the increase of girth of the chest observed by Armieux and by Kellett, may not this have been merely the result of returning health in the invalid? It may have had no connection with diminution of atmospheric pressure.

What are the ordinary obvious effects of a change from low country to considerable heights? In the first place they are distinctly of an exciting nature. The great majority of mankind have immense confidence in the pure and bracing atmosphere of the hills, and there is commonly for the first few days a certain amount of excitement of a complex origin. High spirits and increased appetite are usually the first result of a visit to the mountains. We have before us a letter from a friend who has been travelling in Ladakh. He writes that the tableland is seldom lower than 15,000 feet—"For three weeks we camped at over 18,000 feet, and shot all about us. The climate is splendid, the air rarefied and bracing. I never enjoyed myself so much, and never recollect having such an appetite." Such may be the effects of elevation (here perhaps a little overstated) on healthy constitutions, but the mountain climate of much smaller elevations is by no means always so favorable in its operation. It frequently produces headache and sleeplessness; and constipation, or more frequently diarrhœa, are the result of the

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but that after ten months' residence more air was inspired and more carbonic acid expired.

sudden influence of cold on the action of the liver. We have known officers who resided in the Himalayas at a height of 6000 feet, but whose duties sometimes took them to villages up at the height of 12,000 feet, complain of a feeling of constriction at these heights, and always feel glad when they found themselves again at the lower elevation. The first effect of excitement soon passes off in those who have come up from the plains, and living at heights such as 4500 feet in Europe, or 7000 feet in the Himalayas, is bracing to most people, although there are always a certain number of individuals, chiefly excitable ones, with whom the mountain climate does not agree—a fact which is manifested most frequently by unpleasant nervous symptoms.

On the whole, a return from the climate of the hills to that of the plains produces less disturbance of the constitution than might have been expected. In the cold weather in India a European regiment has often descended from the hills, and entered at once in high health on a campaign; and even in the year of the Mutiny, three regiments came down from the hills to Delhi, and notwithstanding their forced marches in the hot winds, there would not have been anything amiss in their health but for their having come on the track of cholera.

Perhaps those who descend from the hills are more apt to contract fever in passing through malarious districts at their base than those who are ascending. In Mexico it is observed that the residents of the high plateaus are particularly apt to get yellow or other fevers on descending to the sea-level.

In regarding the influence of altitude on man, it ought to be a matter of much practical importance to ascertain what diseases are most prevalent at different altitudes, and it might be possible to construct comparative tables of altitude and disease. But as you cannot from the mere latitude and altitude of a place determine its temperature, so mere altitude does not absolutely determine the climate which again influences disease. The local configuration, the presence of hills, or lakes, or forests, the race of mankind to which the inhabitants belong, all have a modifying effect, as well as winds and moisture.

Our best plan, therefore, is to group together certain high lands which are geographically connected, and observe what diseases have been found to prevail in them. We shall take some districts of India first. In the lower districts of the Himalayas up to 7000 or 8000 feet are to be found typhus, sometimes of a bubonic type (true plague is believed never to reach such heights), smallpox and measles, dyspepsia, diarrhoea; rheumatism is very frequent, cholera occasionally prevails. The hope that elevation was a safeguard against cholera is now abandoned. In a few districts there is leprosy, and in a vast

number of localities goitre is common. Up to 3000 feet and more malarious fevers are frequent.

Above 4000 feet malarious fevers are very infrequent, and though inflammations of the chest are not uncommon, it is probable that phthisis is rare, but our information is scanty.

The plateau of Thibet lies at a height of about 11,000 to 12,000 feet. In Ladakh goitre is frightfully common. Leprosy is not unusual: there is a great deal of catarrh; smallpox is at times very destructive. Ophthalmia and snow-blindness are frequent.

Descending from the high plateau of Thibet we come on Kashgur, at an altitude of 3000 to 4000 feet. Mr. Bellew gives a wretched account of its unhealthiness. He observed much dyspepsia and melancholia; goitre was common, so were scrofula, a form of cancer, and phthisis; but intermittent fever was rare.

Marco Polo was struck with the immense quantity of goitre at Yarkand at a similar elevation, for which it is at the present day remarkable; and Colonel Yule remarks that the theory of close mountain valleys being a cause of it does not apply here, where it is so frightfully prevalent on an open plateau, as it is indeed in other parts of the world, away from mountains.

The high land of Mexico, of the elevation of 7000 feet and somewhat more, has its full supply of ailments. Dyspepsia and diarrhoea are common, so also are convulsions and apoplexies. Leprosy is not infrequent, is described as of three forms, and is probably of independent origin in the country; rheumatism is frequent; asthma is occasionally met with; eruptive fevers are common; attacks of liver ending in abscess are not rare.

Yellow fever does not rise above 2000 feet, and intermittent fever is rare. Catarrh and bronchitis are frequent, but phthisis is uncommon.

About the very elevated portion of the United States we have only gleaned a few facts; as that there is extremely little phthisis at New Mexico. At Fort Bridges, Wyoming, they have at the height of 7100 feet a sort of mountain fever. At Fort Garland, Colorado, 8365 feet, hæmorrhages and menorrhagias and uterine disorders are common, and at Fort Fetterman, Wyoming, 8500 feet, affections of the throat and eyes are frequent.<sup>1</sup>

If we now turn to the South and to Peru the following are a few of the facts concerning its great elevations.

In the region from 4000 to 11,000 feet high, a spotted leprosy

<sup>1</sup> The Americans have projected an observatory on Pike's Peak, Colorado, at a height of 13,000 feet.

and the verruca, a sort of bleeding frambœsia, and a curious carcinomatous affection of the scrotum are common. In the highest region of 11,000 to 14,000 feet (which, by the way, has been actually reached by railways), inflammations of the brain and lungs are frequent. Erysipelas is common, also the peculiar mountain asthma called *Mal de Puna*, which has some analogy with sea sickness. Various cutaneous affections and inflammations of the eye are common. Here occurs a peculiar febrile disease with cerebral symptoms called the *tabardillo*. There is scarcely any phthisis, though it is common below. Hepatitis is extremely rare.

Bolivia.—There is goitre in the valleys up to 3000 or 4000 feet.

Returning to the old world, the tableland of Abyssinia, from 5000 to 9000 feet high, deserves notice. The country on the whole is healthy, but there are frequent affections of the organs of respiration; tapeworm is very common, so also are dysentery, leprosy, and ophthalmia. Scrofula is common and phthisis occurs occasionally. If we come a little nearer home, we find that there are scarcely any permanently inhabited places in Europe of the height of 6000 feet, or it might be more correct to say 5000. The great St. Bernard has been almost the only exception, but we are likely soon to have another in the observatory which the French are erecting on the Pic de Midi de Bigorre, at a height of 8700 feet, or 1200 feet higher than the Great St. Bernard.

What we know of the diseases of the St. Bernard is mainly this,—that the monks break down after seven or eight years' residence, that asthma is exceedingly common among them, and that they have to descend to a lower level.

The main affections of the higher inhabited Alps are pneumonia, pleurisy, catarrhs, a contagious pleuro-pneumonia bronchitis, and asthma, also sunstroke. Rheumatism is common at all heights. Anæmia, chlorosis extending to hæmorrhage (but not to hæmoptysis), are frequent. Tuberculosis, scrofula, and rachitis are absolutely absent from the Upper Engadine. Goitre and cretinism are very abundant in certain districts, and this is said only to be in the case of some valleys. Intermittents are rare. Phthisis is undoubtedly rare, but at elevations of not more than 3000 feet it is quite common, and the health of the lower half of the inhabited portion of the Alps is not particularly good. Indeed, Lombard describes this intermediate zone as suffering especially from phthisis and typhus. Eruptive fevers when introduced rage at any height in the Alps, just as they do in the Himalayas, in Abyssinia, in Mexico, and in Peru. Descending a step lower, we find that the two

European capitals, which are situated at heights approaching 2000 feet, Munich and Madrid, are by no means remarkable for their salubrity—pneumonias being amongst the most fatal of their acute maladies. From this long list of diseases which are met with at high elevations we are not to infer that such altitudes are unhealthy, but that they are not so exempt from disease, as those who always paint mountains in *couleur de rose* are apt to imagine.

After this survey we naturally ask the question, Have the mountain climates any permanent immunity from any disease?

The answer seems to be, that they afford a positive immunity from yellow fever, and probably from the plague, and a very considerable immunity from intermittents; and that phthisis is undoubtedly rare, although positive freedom from it cannot be attributed to any climate, and that such exemption can only extend to the tubercular form of the disease. The more the question is examined, the more certain does it appear that mountains in general cannot be said to enjoy entire immunity. Jourdanet makes the line of exemption commence half way between the sea and snowline. It must be remembered that in many countries where phthisis has been supposed to be almost unknown, as in the East Indies, further researches have dissipated the notion. There are obvious considerations which make it probable that a disease eminently a social one should not be common in hill populations which are sparse, and where masses of people are not aggregated for manufacturing purposes. Although the impression is general that phthisis is rare in the Himalayas, we have heard that it is not uncommon among weavers at an elevation of 4000 feet, but our information on the subject is exceedingly imperfect.

We shall next give a short *résumé* of the opinions that have been held respecting the curative influence of mountain climates on disease. They are mainly the results of the observations of military surgeons on their soldiers.

In Peru, where the sanatoria lie very high—at 10,000 to 12,000 feet—they send down rheumatic cases to the plains. On the other hand, they send up cases of remittent and of intermittent fever, which soon lose their periodical character; also cases of bilious diarrhœa, of dysentery, and of hepatitis. We have no very distinct account of that class of cases, but they are said to mend. There is positive evidence as to the wonderful benefit derived from sending cases of phthisis up from the sea-level.

We have fuller accounts of the experience of the French in sending up their sick to heights of about 8000 feet in Mexico. The mountain climate had a beneficial effect on intermittent

fevers, and also on diarrhœas and dysenteries ; but occasionally diarrhœa turned into dysentery, and cases of dysentery became hæmorrhagic ; the cases of dysentery had seldom hepatic complications. The fresher the cases were the more benefit they derived ; old cachectic conditions made much less improvement. But even those who do not admire the Mexican climate, admit that the French soldiers were at least as free from disease in Mexico as in France.

The sanatoria of the East Indies are usually at from 6000 to 7500 feet of elevation. Almost all cases of consequence from acute illness mend rapidly, especially from fever, where there is no visceral enlargement. There are instances of cases of phthisis that have done well. Women and children of feeble or chlorotic condition usually improve fast, also some cases of hysteria and of nervous affections ; but for some of the latter the climate may be too exciting. To those suffering merely from overwork a visit to the hills is usually most advantageous. Chronic diarrhœa and dysentery, visceral enlargements, and syphilis do not do well, and cases of dyspepsia seldom derive more than temporary benefit.

If a regiment has suffered in the plains from malarious poisoning, it gets entirely rid of its effect in two seasons. European soldiers employed on public works at a height of 7000 feet, enjoy the most wonderful health. Regiments of Europeans usually are exceedingly healthy in the Himalayas. The good effects are more marked in the second than in the first year. Of a longer residence than one or two years we have no account. It would be very desirable to know what would be the effect of a more protracted residence. English children thrive in the Himalayas.

Swiss and Pyrenean sanatoria vary from about 3000 to 5500 feet in elevation.

They are particularly suited for convalescence from acute diseases, for cases of chlorosis and anæmia, especially when the latter is the result of fever or of tropical disease, for slighter cases of diarrhœa and of dysentery, also for indigestion, for catarrhs, humid asthmas, and for scrofulous conditions. On the whole, for phthisis generally. A short visit to Alpine heights appears to be in all countries especially refreshing to those who are worn out by business or other cares.

If we next inquire what effect permanent residence in mountains has on the human constitution, we must be careful not to confound mountaineers, who, though they may live among mountains, do not live at any great altitude ; for instance, the highlanders of Scotland, with races of men who live at great altitudes. Jourdanet draws, on the whole, an unfavorable picture

of them ; he describes them as anæmic, sluggish, and apathetic, and he refers to the natives of Mexico, of the Cordilleras, of Abyssinia, and of Thibet, all of whom live at an elevation of more than 7000 feet, as illustrations. We confess that he shows considerable reason for his assertions, although he should not have included the wretched Indians, who drag out a miserable existence in the sulphur mines at Popocatapese at a height of 17,000 feet, whose case is quite an exceptional one.

Bellew and others describe the natives of the Asiatic plateaus as, on the whole, a poor and lazy race ; they seem all incapable of any great mental or, indeed, physical exertion. Whatever amount of truth there may be in this, it has long been the received opinion, on the other hand, that mountaineers are fine races and at least equal in mental and physical qualities to their neighbours of the plains.

Jourdanet, when he looks back at the signs of a higher civilisation among the Peruvians, is inclined to regard them as in a state of progressive decay, which he attributes to the effects of elevated residence, and he boldly prophesies a rapid termination to English dominion in India, if we should be misguided enough to transfer the seat of government to Simla. In many ways we believe that such a measure would prove highly injurious, but not merely on account of the diminished atmospheric pressure of the air of that sanatorium ! Government officers do not work at Simla as they do at Calcutta ; but then the life at the former place is avowedly a sort of holiday or watering-place life.

As yet we have said nothing of the effects of mountain climate on animals, though a study of it is instructive.

Although the life of the lower animals is maintained at great heights, and the Condor rises in the Andes to 23,000 feet, and butterflies and fleas are to be seen in the passes of the Himalayas at 18,000 to 19,000 feet, yet the domestic animals do not thrive in high regions as well as in low ones ; they generally deteriorate in size, and are incapable of doing the amount of work done by animals in the plains. Here again we allude to really Alpine heights, not merely to mountainous districts. In the higher regions horses and mules very readily get sunstroke, and a little over-exertion often produces attacks of emphysema. In Peru the animals suffer, like men, from the peculiar *mal de montagne*, and in the Himalayas even the enduring yaks sometimes succumb. All the highest mountain passes are strewn with the bones of beasts of burden.

The subject the outline of which we have been tracing, if followed into all its ramifications, suggests various interesting questions for consideration to which we have been able barely



to allude—such as the effect of mountain climates on their inhabitants—how far not only their physical but their mental characteristics are influenced by it. Then there is the question how far Europeans could colonise mountain districts in the tropics. The little we see of European labour on the part of soldiers in the Himalayas does not accord with Continental experience that workmen at a height of 7500 feet in the Alps get worn out in twenty years, while those who live lower down may work from thirty; that the men above require much more food for their sustenance than men below; on the contrary, English soldiers have looked well and have enjoyed high health; but apart from this, European ordinary labour, that is, tilling the fields, or cultivating the unproductive sides of the mountains, could never prove remunerative, and such a population could not be self supporting. There is also the very interesting subject of hill sanatoria, which are springing up in all parts of the world, and are becoming every day more and more frequented. Of the wonderful advantage of hill climates to our soldiers and women and children, as also to the higher classes, we have now had lengthened experience in our Indian possessions, and the public in Europe is gradually awakening to the fact that advantages of a similar nature, though scarcely so marked, may be obtained by visits to the mountain elevations of Europe.

On many of the subjects on which we have been obliged to speak doubtfully, light is being gradually shed, and we look to India to furnish us with clearer and more precise information on the mode of action of mountain altitudes on man in health and in disease, than it has hitherto supplied. We trust that Dr. Kellett will soon not stand alone in his observations on such subjects, and that we shall not be much longer dependent on foreign authorities for our physiological data.

The study of mountain climate must be a popular one at present, or a Parisian publisher or author would scarcely have undertaken the risk of introducing such an elaborate work as that of M. Jourdanet to the world.

JOHN MACPHERSON, M.D.

III—Spence and Bryant on *Practice of Surgery*.<sup>1</sup>

THE almost simultaneous appearance of the second edition of these two large and representative works on surgery, tempts an endeavour to draw a comparison between the teaching and practice of the two great schools from which they emanate. Professor Spence may fairly be claimed by our friends over the border as one of the best specimens of the great school of surgery of which Liston, Syme, and Fergusson were members fifty years ago—a school that, uniting boldness of design with extreme simplicity and directness of execution, gave a great impulse to surgical practice, and did much—far more than the present generation are apt to remember or give them credit for—to remove from surgical practice the reproach of being a farrago of old wives' nostrums, and to render it at once a logically well-founded science and a brilliantly executed art.

His large and prolonged experience as a teacher of anatomy, surgical anatomy, and surgery, his single-eyed devotion to surgical work, not as a trade by which money may be made, but as itself the one great end of an existence to a reasonable being, and finding in successful results the only legitimate or desirable reward, make us certain that any work of his will be not a mere "pot-boiler," but a true labour of love, to be lingered over and retouched, to be lightened up by whatever rare and quaint little bits of fancy, poetry, or philosophy he may light on, will, in fact, be very much the reflection and outcome of his life. So far as this goes we may accept Professor Spence's book as representing him, and consequently the school of which he is about the only survivor. But will it also represent the Edinburgh school of the present with sufficient accuracy to enable us, by comparing this work with that of Mr. Bryant, to compare the Edinburgh teaching with our own in London? We believe it does, especially on the practical side, which only we mean to take up in this notice.

Surgical principles, the doctrine of inflammation, the pathology of the healing process, the classification of tumours, are all undergoing revision and alteration from day to day, and there is a school in Edinburgh as well as here who use a language,—dare we call a jargon, nearly unintelligible to some of us older ones—aseptics and antiseptics, germs that putrefy and germs that keep sweet, clots that organize and others that

<sup>1</sup> 1. *Lectures on Surgery*. By JAMES SPENCE, F.R.S.E., Professor of Surgery in the University of Edinburgh. Second edition, 1876.

2. *The Practice of Surgery: A Manual*. By THOMAS BRYANT, F.R.C.S., Surgeon to, and Lecturer on Surgery at, Guy's Hospital. Second edition, 1876.

disorganize, pus and putrefaction, sweetness and serum; to these penetralia Mr. Spence has no access; to this culture he is as great a Philistine as the most benighted of us in London.

But in the more practical regions, in the mode of amputating, in the principles on which vessels should be tied, on the after-treatment of patients, Mr. Spence's dicta may still, we believe, be held as fairly representative of the great surgical school which teaches in such freedom in Edinburgh, but is all united in practice under the one roof of the Edinburgh Royal Infirmary.

Can we, on the other hand, accept Mr. Bryant as a fitting representative of the English or London school of surgery? Well known for years as a careful and industrious writer on clinical surgery, he has, after being a lecturer on systematic surgery, essayed the very difficult task of writing a text-book. We all remember with what spirit and vehemence the first edition was criticised, in some quarters even abused; still, when the smoke cleared away, Mr. Bryant was found still alive, and his book has now reached a second edition. Inaccuracies of language of the most glaring kind, ludicrous grammatical errors, and an utter want of the sense of humour, literary power, and even of arrangement, spoiled the first edition. Some of these deficiencies remain, though the critics have helped to correct mistakes. Still, all of this is outside work, the essentials remain, and the work gives a fair, easily accessible, and very full account of the surgical practice of one of the largest, oldest, and most prosperous schools in London. Aiming at no lengthened or even complete exposition of surgical principles, it has evidently been the author's aim to omit nothing of surgical practice that the student and surgeon could possibly need; in this respect it is much fuller and more encyclopædic than Mr. Spence's work, and at many points overlaps and supplements it.

Commencing with the great centre and foundation of all surgical principle, the inflammatory process, or doctrine of inflammation, we find in both our authors an evident determination to be brief and practical, and to avoid theory. This is wise at the present stage of our pathological knowledge; all theories at this time being on their trial, in consequence of the immense progress lately made in microscopical work, especially in the direction of tissue staining and the examination of parts and processes *in situ* and in the living animal, rather than when teased out and after death. While Mr. Bryant devotes a few pages to the process of repair by granulation, so important in surgical practice, Mr. Spence endeavours, in a somewhat discursive and gossiping lecture, to give the student a general idea of some of the chief theories that in the past have been used by

surgical teachers to conceal their ignorance of exact pathology by a mist of ambitious words.

That this general idea is not overloaded or concealed by details is evident enough when we find in two and a half pages the various stages of Boerhaave, Cullen, Macartney, Bell, Bennett, Virchow, Cohnheim, and Stricker briefly hinted at.

In the treatment of inflammation we find a very general agreement; both authorities give a similar sound as to the question of venesection or general bloodletting. Mr. Spence, after presenting an account of the action of general bloodletting, gives an apologia for his evident belief in its value, and states—

“I can remember many a case where relief from suffering was afforded, and cure of acute disease effected by the prompt and judicious use of bloodletting” (vol. i, p. 12).

Mr. Bryant also says—

“At the present time the practice of venesection is a very rare one; indeed, at Guy’s Hospital it is as rare as, if not more so than, amputation. Forty years ago it was one of the commonest, and there seems some reason to believe that it will soon be practised again with greater frequency, particularly when we find Sir James Paget asserting that we undoubtedly overvalue the blood, and estimate too cautiously the loss of it” (vol. i, p. 482).

Mr. Spence prefers the median cephalic, Mr. Bryant also the median cephalic, if it is of a good size.

An interesting example of the manner in which personal experience and predilection modify surgical teaching is found in the account given by each author of the subject of abscesses round arteries and the resulting danger of hæmorrhage. Mr. Bryant dismisses the subject thus:

“When veins and large arteries open by ulceration into abscesses, an accident of occasional occurrence, they must be dealt with on the principle laid down in the chapter on hæmorrhage,” in which there is no further special reference.

Mr. Spence gives a most valuable and exhaustive though concise account of the whole subject, including the following practical advice:

“This danger [of ulceration] should always be borne in mind as a reason for promptly opening abscesses near vessels, even if on opening such an abscess nothing but pure pus should escape at the time; you must not consider this to be conclusive evidence that the vessel is unaffected, but watch the case carefully, for after a lapse of six or eight hours the artery or vein may give way, and serious hæmorrhage result. This is probably caused by the weakened coats yielding to the force of the circulation after the equable fluid pressure which surrounded them has been removed, or may, as stated

by some, be due to the entrance of atmospheric air, which hastens putrefaction, and the consequent bursting of the coats of the vessel. At all events, one thing is evident, namely, this, that abscesses so situated should be opened early, for otherwise the veins and arteries are in great danger. If bleeding does occur in such a case the best treatment is to tie the vessel above and below the point where it has given way, or if the arteries be small and deeply seated you may succeed in arresting the hæmorrhage by pressure" (vol. i, p. 35).

On the causation of hospital gangrene both agree that want of cleanliness, foul air, &c., are factors. Mr. Spence notices that the best wards are not unfrequently those in which unhealthy conditions manifest themselves, and the cause is not far to seek; being the best, they are the wards in which for obvious reasons we place our operation and other important cases, and after a time our favourite ward becomes the scene of some unexpected unhealthy condition. Hence the advantage of cleaning out and fumigating all wards for a few days at intervals. For local treatment both are at one on the advantage derived from the use of the strong mineral acids. Surely it is a relic of the middle ages when Mr. Bryant solemnly tells us "*No sponges should ever be employed.*" We would hope that no sponges ever are employed in Guy's Hospital wards. If they still are we have not far to seek for *one* cause at least of hospital gangrene.

In senile gangrene, our authors are scarcely at one regarding treatment of the part. Mr. Bryant says, "When small parts are alone implicated, their separation may be left to nature; but where hands and feet are involved in the gangrene the surgeon should assist nature's processes by amputation above the line of demarcation or close to it, as soon as it is indicated." Mr. Spence says, even after a line of demarcation has formed we should not interfere, beyond dividing through dead textures to remove the fœtid mass and diminish the risk of absorption of septic matter. We should let nature form a stump of her own. As regards the management of the patient, the old-standing difference between London and Edinburgh practice is still apparent, for while Mr. Bryant says that "abundance of bland nutritious food should be given, and stimulants with tonics to assist the digestion and keep up the circulation (p. 41.):—Mr. Spence describes this treatment only to disapprove of it. "The treatment was formerly based on the idea that the disease was due to debility, and that the patient therefore required great stimulation; thus, strong soups were given, with much animal food, brandy and strong wines, and large quantities of powdered Peruvian bark

as an antiseptic. . . . The proper course is to allay the irritability by the use of opiates internally, and to avoid all over-stimulation of the patient by giving simply enough for nourishment and this of a kind easily digested, such as farinaceous food and milk. No bark should be given, at least not in the earlier stages" (pp. 66-67.)

Taken in connection with the above we find a most interesting and suggestive discrepancy in opinion as to the pathology and treatment, local and general, of another serious malady—carbuncle. The northern surgeon describes it as an inflammatory disease of the skin, serious in its character, attended with great constitutional disturbance, and not unfrequently terminating fatally. The southern surgeon finds it in feeble and cachectic subjects; that it is slow in its progress, and that, as a general rule, it is not a fatal disease. Mr. Spence treats it by "making free crucial incisions into and beyond the diseased part; by so doing we will not altogether prevent sloughing, because from the very commencement of the disease sloughs are almost sure to be present, but we prevent the disease from spreading in surface and depth, and also permit the free escape of the slough which keeps up the irritation." The incisions must be carefully made; they must go completely through the inflamed textures, and beyond them into the healthy skin, or otherwise the disease will extend from the circumference. Though smaller incisions may relieve the pain and tension for a time, they will not cure the disease. *No matter how weak the patient be, the incisions ought to be free* (p. 81).

On that very point Mr. Bryant says, "In former days the one form of local treatment that every surgeon followed was that of the crucial incision, the knife being passed freely through the tissues to the base of the inflammatory effusion; the object of the incision was to give room to the slough to separate and come away. In modern times the value of this practice has been much questioned, for it was too often found to be followed by loss of blood where blood was much needed; and it has been questioned whether the incision did much to hasten the progress of the case and the separation of the slough. "I was taught it, but from observing its effects have long given it up, believing that it did little or no good and was often followed by a harmful hæmorrhage," (p. 169).

To this Mr. Spence has his answer ready, almost as if he was meeting an argument:—"The incisions are apt to bleed very smartly, but we need not be afraid of this . . . an ordinary carbuncle in back or hips seldom goes deep enough to involve any large vessel." He points out, however, that in certain

weak subjects, especially if albuminuria is present, a slow passive hæmorrhage may be troublesome and even dangerous.

Now, may there not be some reason for the great discrepancy in the opinions of these careful and sagacious surgeons upon a point of such importance? May there not be some real difference in the constitutions of the patients we meet with in London and Edinburgh? Is it possible that Scotchmen bear depletion and starvation better than the already weak and anæmiated patients we meet with in London?

Mr. Bryant uses as a local treatment the caustic method, as advocated in this country by Pritchard, and in America by Dr. Physic—

"Either by rubbing the caustic potash freely in the centre of the carbuncle until an eschar is fully formed, or, which is preferable, by puncturing with a scalpel and inserting the stick or a small piece the size of a pea; when the carbuncle is large many punctures may be made, one to every surface of the dimensions of half-a-crown. When openings exist the surgeon has only to insert the caustic and let it melt, either in stick, or what is better in pea-like masses, passing them well down into the subcutaneous tissue. By this practice the slough is certainly cast off more readily than when incisions are made or the case left to nature" (p. 169).

As, at the present time, the whole subject of pathological classification, especially of morbid growths, seems in a transition stage, and a destructive criticism has fairly torn to pieces most of the methods of classification that used to be trusted to, we are not surprised to find Mr. Bryant coolly escaping the difficulty by—

"Regarding tumours in their clinical aspect alone, giving their anatomical characters only so far as they illustrate the practical aspects of the subject. All speculative pathological doctrines will be put aside, as tending to confuse rather than elucidate clinical phenomena—waiting for the day when pathological science shall have so far advanced as to allow of an anatomical classification of tumours being made, that will dovetail in fully with that founded on clinical observation, (p. 97).

For further information on the pathology of tumours he refers his readers to Virchow on 'Cellular Pathology,' and on 'The Microscopical Anatomy of Tumours,' to the chapter Dr. Moxon has written for this work, p. 97.

Mr. Spence on the other hand, believing that every pathologist has a structural classification of his own, finds it necessary to select from these one which is at once simple and exhaustive. Mindful of the old alliance of Scotland with France he finds that in the arrangement (slightly modified however) proposed by Cornil and Ranvier in their *Histologie Pathologique*,

which is based upon the analogies which the structure of a tumour presents to the structure of normal tissues.

Very different in the case of both surgeons is the evident zeal and interest in the practical points bearing on the diagnosis and treatment of tumours, from the perfunctory and careless manner in which as we have seen they dismiss the pathological arrangement of them. In both works the principles of management of cancerous breasts are given with great accuracy. In both there is a very distinct permission to operate even in cases where lymphatic glands in axilla are affected, which is in marked contrast with the teaching of even twenty years ago. Both prefer the knife to caustic and both would forbid operation in cases where the skin is involved, red, brawny, or tubercular. Mr. Spence approves of the principle of subcutaneous injection of weak acetic acid, while after a trial of it in twenty cases, Mr. Bryant has never found any good results ensue from it.

On the vexed question of cachexia, Mr. Bryant asks the question, is there a cancerous cachexia—and answers the question as follows :—

“I have little hesitation in stating my belief that in practice no such thing can be established, and that a large number of patients suffering from cancer are as healthy looking as any other class, indeed often healthier. There is no doubt that a patient suffering from cancer, which, by its discharges or development, is interfering with the important functions of life, and undermining the patient's powers, has an anxious, drawn, bloodless, and waxy look; but so has the subject of any organic disease which interferes with the functions of digestion or assimilation, and particularly so the subject of rectal disease. The patient exhausted by suppuration, by spinal, bone, or joint mischief, the man or woman who from drink, syphilis or mercury, separately or combined, is gradually being brought down to death's door, all these have a cachexia more or less peculiar according to the organ involved in the disease, and the special character of the patient, but it is merely the cachexia of looking ill. Clinically I read the meaning of a cachexia as a looking ill, perhaps very ill, but it has no other definite signification, looking ill from cancer, looking ill from abdominal, rectal, suppurative or syphilitic disease, p. 6.”

Mr. Spence on the other hand evidently believes that true carcinoma is a constitutional disease, and that the general health of the patient is always affected sooner or later and generally from the beginning. The patient has an exhausted and anæmic appearance—the cheeks are sunken and there is a yellowish colour of the skin, with a quick pulse, and this even in the early stage (p. 105).

When we come to the subject of the general treatment of wounds we find a very considerable difference in the method in



which our authors consider the subject. Both are as one probably as to their practical management of cases, both seem to recommend extreme simplicity of dressing, and both strongly approve of free drainage, so that serum or pus may readily escape. While Mr. Bryant says comparatively little on the history of the subject, and dismisses the methods of treatment in a few sentences, the northern professor takes the opportunity of making some general observations full of quaint humour, and qualified with a dash of good-humoured sarcasm. He tells us of the older surgeons who assisted nature by unguents and cataplasms, and vindicates for them the title which John Bell had denied them of "philosophers," or "philosophical surgeons."

"If," says Mr. Spence, "the proper study of mankind is man," then assuredly on that important study these men had founded their philosophy. They had noticed that patients in general feel rather flattered by something special being done, some little halo of mystery to brighten their sufferings and give importance. They said that for one 'Good Hezekiah,' who meekly submits to so simple an application as a 'poultice of figs,' they would meet with a dozen Naamans 'ready to turn away in a rage' if there was not some great thing done, or some fuss made about their cases, "and so whilst they treated their wounds simply, or left them to Nature, they amused or distracted the attention of their patient by enacting various little incantation scenes or mysteries." After alluding to these, Mr. Spence goes on to tell of the excuses they made in case of failure, showing it to be an accident and no fault of the system.

A "finer spirit had escaped," just as one might say in these later days, a wandering germ-cell had escaped destruction." With a cruel and mischievous humour he tell us that "Were I the patient, I would decidedly prefer that the surgeon should bestow his attention on the blood-stained rag, leaving the wound 'clean and cool,' or swathe the knife which had inflicted the wound in any amount of cabalistically-prepared bandage, rather than that he should wrap up and over-heat the wound with fold after fold of dressing and greasy cerecloth bandages, compressing the parts, and confining the bloody and serous discharges. In a word, I would rather that his incantations were performed on something else than my wound." (p. 139).

This occurs immediately before a brief allusion to the method of treatment known as Lister's, and Mr. Spence takes the opportunity of pointing out what the enthusiastic and apparently juvenile admirers of this system seem too apt to forget, that by simple dressings extraordinary successes have been obtained,

which as yet at least had not been equalled far less excelled. Mr. Spence in three years out of sixty-three major amputations for disease had only three deaths, and out of twenty-three cases of excisions of joints only two deaths, when his treatment consisted merely of washing the stumps or wound with tepid water, and the only dressing was a veil of muslin or lint over the stump.

Here, in London, we believe that nearly all surgeons will find that the open treatment of wounds with extreme care as to cleanliness and free egress to the discharge, has practically superseded all other modes, and we will wait the further publication of results of the more complicated dressings prior to adopting them.

On the important question of treatment in gunshot wounds involving fracture of the thigh, the practice of both authorities seems similar, amputate in cases where the middle and lower thirds of the limb are injured, if there is any doubt as to the integrity of the great vessels, while if the injury is in the upper third the patient has a better chance under expectant treatment than with the great risk of primary amputation at the hip-joint.

In common with all surgeons of long hospital experience, neither Mr. Spence or Mr. Bryant are at all fond of the operation of removing loose cartilages from the knee-joint. Mr. Spence says, "There is no class of operations that I have a greater dread of than the apparently simple one of removing a loose cartilage. The case may go on most favourably, and then when the patient is almost well inflammation and suppuration may set in." (p. 261.)

Bryant says, "In cases where direct incision is feared, *and this ought to be in all*, the removal by subcutaneous incision may be employed. . . . But Larrey's statistics prove that the operation is a serious one, when practised by direct incision, and a difficult one by the subcutaneous method, that extraction is more dangerous than the persistence of the affection." (p. 475.)

Younger and more sanguine surgeons will also be surprised, almost pained, by the verdict of both these experienced men as to excision of knee-joints. Mr. Spence first limits the class of cases suitable for the operation, for after saying we must not wait too long before operating, that means we must pick our cases, he says that most cases of gelatinous degeneration in the knee are not well suited for excision. The operation does not seem to answer so well here as in some diseases of the articular ends of the bones; in most cases the general health of the patient is not so well suited for the operation, which requires considerable strength and involves a long period of convales-

cence." Having then picked out cases, taking them early in the disease of the least severe type, and only those whose health is not much affected, we would surely have a right to expect a good result in the statistics, but what are we told :

" Were it done in all cases in which amputation used to be performed, we would find that the statistics of excision would be much less favourable than those of amputation. When we contrast the amputation performed for disease of the knee-joint with excision for the same disease, we find as a general rule that the former are much more successful—the deaths from amputation being 1 in 7, those after excision 1 in 3 or 4." (vol. i, p. 277.)

Mr. Bryant goes much further in the same direction. He finds some very striking results by comparing the operations as performed at different periods of life. In 69 cases of amputation for disease of the knee only three deaths, or 1 in 23. In 97 excisions of the same class at the same period 27 died, or 1 in 3½. *Excision being thus nearly seven times as fatal as amputation during young life.* In 119 amputations between 21 and 40 for chronic joint diseases 38 died, or 1 in 3; of 74 similar excisions 39 died, or more than 1 in every 2.

With much reason he asks are the advantages of excision therefore so great as to justify a surgeon in submitting a patient to an extra risk in order to secure them? As hitherto practised, and as a general rule in surgery, I have no doubt in answering in the negative. (vol. ii, p. 462.)

These doctrines ought to impress the profession. Without dogmatising, our own view is that in selected cases between 18 and 25 years of age, excision may be done as an operation of expediency to save the limb; but it should never be performed as an operation when the patient's *life* is in question, or in cases where any organ has given evidence of weakness, or in which a confinement to bed for four months at least is evidently a risk. Fairly fat young women, with few or no sinuses, and suffering from articular disease, not synovial degeneration, are by far the best subjects for excision, and in them with care a fair percentage of recoveries may be expected.

Fractures of thigh are often troublesome, always tedious; methods of treatment vary with every school. Mr. Spence gives a most cordial and unhesitating preference to extension by weight and pulley, with the use of lateral Gooch splints when needed. Mr. Bryant approves of a method combining suspension and extension, which is a modification by Mr. Johnson Smith of a method devised by Dr. Hodgen, of St. Louis, United States of America.

Coming now to hæmorrhage and hæmostatics, both our authors report favourably of the use of Esmarch's method,

which he calls the "bloodless method." Neither approves of it in amputation, and both find its chief value in operations for necrosis or excisions of bones and joints, especially where no great vessels are cut, and plugging or pressure is sufficient to stop the subsequent hæmorrhage. When vessels have to be sought for and tied after the tubing is removed, so much oozing takes place that in the end little blood is saved to the patient.

"*Torsion*," says Mr. Spence, "has recently been revived, and attempted to be introduced as a substitute for the ligature or acupressure. It answers well enough for small vessels, and I frequently use it. Past experience, however, has shown that we cannot safely trust to it in the larger arteries." (vol. i, p. 444.)

Mr. Bryant says—

"In a physiological point of view there is no method at the surgeon's command more perfect for the control of hæmorrhage than that of torsion. . . . The physiological arguments in its favour are very great, and the practical advantages seem to be no less. After seven years' experience of the practice applied to vessels of all sizes, the femoral being the largest, I have had no mishaps. . . . I have had stumps heal in a week and patients up in two weeks without one single drawback. At Guy's Hospital up to 1874 we have had 200 consecutive cases of amputation of the thigh, leg, arm, and forearm, in which all the arteries had been twisted (110 of them having been of the femoral artery), and no case of secondary hæmorrhage." (vol. i, p. 409, 410.)

A strangely different result, and one hardly to be accounted for by the different method. Mr. Spence continues the twisting till the twisted portion comes off. Mr. Bryant only rotates the end till the sense of resistance has ceased, and says the end should not be twisted off.

Few subjects seem to embarrass and confuse a student at an examination more than the pathology and varieties of aneurism, especially the distinction made between true and false aneurism, according to the number of coats involved, the nature of contents, shape, &c. Mr. Spence cuts the knot which has puzzled so many by classing under the head of true aneurism all aneurisms arising from disease of the arterial texture, whilst a false aneurism he defines as nothing more or less than a wounded artery, the blood, prevented from escaping externally, being coagulated in the cellular tissue, which becomes condensed, and forms a sort of cyst, though generally a very imperfect one. The coats of the vessel may be perfectly healthy, and accordingly we can treat it just as we would a wounded artery, by laying open the sac and tying the vessel above and below the wounded part.

Bryant teaches a quite different doctrine, for he agrees with Holmes, that it is impossible *clinically* to perceive any difference between true and false aneurisms when they come under observation, the true being apt to become false as they grow, and the false being far the more common. He refers, as a matter of history, to the old definition of a true aneurism, in which all the coats are involved in the dilatation, and of a false one, in which the inner and middle coats having given way the outer alone remains.

The arrangement of the northern professor is certainly the simplest, and as the old one is confessedly of little clinical value, might fitly supersede it.

We have looked with much interest for the opinion of these experienced surgeons on the vexed question of anæsthetics. Mr. Spence gives a clear and decisive opinion. He has given chloroform since 1847, and has lost only one case, in which fatty heart was present:

"When the patient has a weak heart and feeble pulse, or is advanced in life, I usually mix a little anhydrous sulphuric ether with the chloroform, or begin by the administration of chloroform, adding the ether if the pulse shows a tendency to flag. The stimulating effect of the ether under such circumstances is very restorative, although for general use the chloroform answers far better" (p. 475).

No precautions are taken as to quantity given, but in all cases of important operations the patient should be placed recumbent. We would add strength to this last most essential point by saying that in all cases of giving chloroform at all the patient should be recumbent.

Bryant, while thinking highly of chloroform, and finding the risk from its administration small, still uses the mixture recommended by the chloroform committee—of alcohol one part, chloroform two parts, and ether three parts—and administers it by a simple mouth-and-nose piece lined with lint, without valves or nozzles. In cases of weak or fatty heart he gives ether alone.

The questions of amputation, methods, statistics of results, and modes of dressing, receive very careful treatment from both our authors, and in many particulars their opinions and practice coincide. One marked point of difference we may note, which, perhaps, is more apparent than real—more one in expression than in practice. It is this: Mr. Spence selects a place where to amputate so as best to secure a sufficiency of skin, cellular tissue, and muscle, to cover in the bone and result in a well-shaped flap and a comfortable stump. Mr. Bryant, with a somewhat exaggerated strength of expression, teaches a very different doctrine:

"To assert that the form of amputation, or rather that the shape of the flaps, &c., has anything to do with the relative mortality of different amputations is incorrect; indeed, what evidence exists points to a different conclusion, for the success of an amputation, as of any other operation, turns mainly upon the condition of the viscera, age, and general condition of the subject, accepting it as a fact, that the older the patient the greater the danger, and the more of the body that is removed the greater the risk" (p. 547).

Hence we find Mr. Bryant advising to amputate through inflamed tissues rather than go higher to amputate at a joint, utilising some injured skin or bruised tissues, and not being disappointed even when these flaps sloughed, the stump subsequently granulating well.

*Ne quid nimis* is a good motto, and probably both Mr. Spence and Mr. Bryant may be trusted; but either doctrine may in practice be carried too far. Parts may, and doubtless have been, sacrificed so as to secure a favourite stump at some "seat of election;" the very name implies such a choice, but also risks to life from putridity of sloughing flaps may be greater than those resulting from an extra inch of limb removed, and we must never forget that a stump healed by granulation is apt to show an adherent cicatrix, on which the patient will never be able to rest, and the length of leg saved may only be utilised as filling up the trouser-leg, which projects behind a knee resting on a wooden peg. We have often had to reamputate those stumps healed by granulation.

Professor Spence's statistics of amputation and its results are of extreme value, as the 503 cases are all his own, and hence the conditions are known, and so far similar, and the distinction is carefully made between primary, secondary, and for disease, of such vast importance as influencing fatality.

They would well repay a lengthened analysis did our space permit. The only point not carefully noted is the question of the bearing of the age of the patient on the result. We have little doubt that it would be found that the mortality increases in a very remarkable ratio with advancing age.

The cases on which Mr. Bryant bases his statistics do not seem to have all been treated by himself, hence lose much of the value which Mr. Spence's have, but his bring out the effect of age with the most startling effect, for—

Out of 103 cases under 20 years of age 10 died, or 1 case in 10.

Out of 111 between 21 and 40 years of age 21 died, or 1 in 5.

Out of 74 over 40 years of age 22 died, or 1 in 3½.

Both Professor Spence and Mr. Bryant agree as to the extreme danger of amputation when undertaken for acute

suppurative disease, such as acute necrosis, nearly all the cases proving fatal.

As to method of amputating, there are, of course, individual differences of opinion in different situations; but there seems to be a very universal consensus in the present day in favour of the mixed mode of amputating, *i. e.* a long anterior flap containing little except skin and cellular tissue, to be brought over the face of a shorter and fuller posterior one containing muscle, and generally also the vessels of the limb.

At the shoulder-joint, which is one of the situations where great differences in the mode of operating are often necessary and allowable, the preferences of the two surgeons are curiously different. Mr. Bryant much prefers two flaps, one from deltoid, the other from inner side of arm. Mr. Spence, in very strong terms, expresses his preference for a deltoid flap or long posterior. Often, however, in this situation, we have to take what we can get and be thankful for it.

In both works pictures are provided of the surgeon and his assistants engaged in an amputation, in both a most unimpeachably healthy looking limb is selected. In Mr. Spence's the solemn gravity, and bewigged dignity of the butchery of the last century is burlesqued. In Mr. Bryant's the calm priggishness and self-contentedness of present day is represented with a fidelity approaching caricature.

In no class of cases do we find a more thorough agreement between our authors, than in those of injury of skull to be treated by trepanning. Mr. Spence dogmatizes so succinctly and well, and Mr. Bryant agrees with his results so closely that we may quote Mr. Spence's six classes of cases in which he would advise trepanning to be performed.

1. *Without delay* in all cases of distinct punctured fracture, to avert mischief by removing the fragments of the inner table.

2. In cases of compound comminuted fracture with depression (not in mere fissure with wounded scalp).

3. In simple depressed fracture, when, after a fair trial of other measures, the urgent symptoms of compression are persistent.

4. In compression from extravasated blood, when the position of the injury, or the existence of a fissured fracture, indicate the probability of a large artery such as the middle meningeal, having been torn.

5. For intra-cranial suppuration, when the symptoms and the existence of the puffy swelling, or unhealthy state of the scalp wound, and bone, give an indication of the probable position of the pus.

6. In certain chronic cases, from disease or alterations in the bone following contusion or other injury, causing cerebral symptoms, such as local paralysis or epileptic fits. This last rule is by no means so imperative as the others (p. 738).

In their descriptions of the operation for harelip, our authors mainly agree, but Mr. Bryant's account is more full and detailed. As to the period best suited for the operation Mr. Bryant prefers the end of the third month, Mr. Spence either early infancy or after the completion of the first dentition. Mr. Bryant absolutely removes as little texture as possible, or in most cases none at all. Mr. Spence says "the mistakes you are likely to commit as beginners are to remove too little texture in paring the edges and omit freeing the attachments of the mucous lining of the cheeks." In most cases of harelip he employs two curved incisions, their concavities being directed towards each other.

When an intermaxillary bone is present, Mr. Bryant prefers to remove it completely, Mr. Spence to break its pedicle and force it into position. Mr. Bryant recommends the use of Mr. Hainsby's truss, so does Mr. Spence, but in rather a lukewarm way, for he says "I have never used it myself, but it has been found of advantage" (p. 785).

One of the most important and most interesting sections in Mr. Spence's work is the one in which he describes the surgery of the air passages, especially tracheotomy in children.

On this subject he has a right to speak, having as he tells us probably the largest experience of any surgeon in this country, having performed the operation one hundred and eleven times in children for different causes.

In several important points Mr. Spence's operation differs from Mr. Bryant's, and probably from most of the surgeons on this side the Tweed, yet where they do differ we rather incline to think Mr. Spence is right. First as to the place of opening in the trachea. Mr. Spence always in children, generally in adults, opens the trachea below the isthmus of the thyroid, between it and the thymus; Mr. Bryant opens it as near the cricoid as possible and disregards the isthmus.

Second. Mr. Spence always clears the trachea thoroughly before opening it; Mr. Bryant opens it as soon as he can feel the rings.

Third. Mr. Spence fixes the trachea before opening it by a small sharp hook; Mr. Bryant prefers his index finger.

On the question of the presence of bronchitis contra-indicating operation in croup or diphtheria, Mr. Spence has seen cause to modify a former opinion, and has noticed that in most of his successful cases it was present, and thinks that it perhaps



in one sense may be considered favourable as indicating a condition of the mucous membrane less predisposed to the formation of plastic exudations.

Mr. Spence's results are exceptionally brilliant. He has operated for simple and diphtheritic croup one hundred and three times and has saved thirty-four of the cases, and yet he does not operate early.

As to the tubes Mr. Spence prefers the old fashioned form with a broad shield. Mr. Bryant a ball and socket instrument, well known as his own invention, which moves with the movements of the trachea and thus does not with its free end make undue pressure on the trachea or vessels of neck.

Of the bold operation of œsophagotomy neither of the authors seem to have had individual experience, but both agree as to the importance of operating on some guide held in contact with the foreign body to be removed, if the foreign body itself is not sufficiently large and prominent.

The clinical cases illustrating the chapter on the surgery of the facial and cervical regions in Mr. Spence's book are admirably told and full of instruction.

Passing on the subject of hernia, we find both surgeons give very full and careful accounts of hernia and its varieties. Mr. Bryant is more precise, full, and has a better arrangement from a systematic point of view; Mr. Spence again is richer in practical hints, results of experience, and in clinical cases. Both give excellent advice as to early operation, and operation in all doubtful cases. Mr. Spence says: 'There used to be a good old general order in the British Navy, issued to all commanders as regarded the propriety of engaging an enemy of superior force, "When in doubt, *fight*, and in regard to hernia I would say, when in doubt, *operate*.'

As to results unless the proportion of private cases in Mr. Spence's list be large, his results are better than the average London Hospital ones. Mr. Bryant tells us that the mortality in hospital after operation for strangulated hernia is about fifty per cent., while Mr. Spence's summary of 127 cases is as follows:—

Nature of Hernia.	No.	Recovered.	Died.
Inguinal . . . .	46 .	38 .	8
Femoral . . . .	77 .	60 .	17
Umbilical and ventral	4 .	3 .	1
	127 .	101 .	26

Of 46 inguinal hernia 4 occurred in females.

77 femoral " 14

Umbilical and ventral were all in females.

The causes of death in the 26 fatal cases were as follows :—

In 17 cases the bowel was distinctly gangrenous.

7 „ peritonitis had commenced prior to the operation.

2 died of pyæmia and mæœna respectively.

With such a large and successful experience as the above, which indeed probably includes little more than half of the cases of hernia seen by Mr. Spence, as it was published fifteen years ago, it is remarkable that he seems to have had almost no experience of either gastrotomy for obstruction or colotomy. He has operated in one case of the former, and does not look upon the operation as one affording much chance to the patient, while we find no evidence that he has ever performed colotomy.

Mr. Bryant approves of gastrotomy, or, as he prefers to call it, laparotomy, and believes it will be more frequently adopted in the future, while he records an individual experience of no fewer than twenty-seven cases of colotomy, four for vesico-intestinal fistula, two for pelvic tumour, and twenty-one for stricture of rectum. In not one case has he ever regretted performing the operation.

But space forbids further details. A word on lithotomy and its results. Spence performs the lateral operation on a curved staff. Of children and boys under twenty he lost only one case ; of adults between twenty and forty only one also ; of forty adults between forty and eighty-two he lost eight. Bryant uses also the lateral operation, but in Aston Key's method on the straight staff. He does not give his own results, but the Guy's Hospital statistics are very remarkable. Of 171 cases in which Key's operation was done at Guy's only three died. This is partly accounted for by the enormous proportion of children's cases, in whom it is well known the operation is almost free from danger, 100 of these cases having been in children under five, all of whom survived.

In stricture Mr. Spence recommends patience, perseverance, and sweet oil ; and in an exhaustive summary of the different methods of treatment gives the palm to vital dilatation in nearly all ; Holt's method in some and Syme's operation of perinæal section in the few that resist the others.

Bryant approves of retaining a catheter, gradually increasing its size in the stricture till a full-sized one can be passed. Perinæal section, either by Syme's method where a guide can be passed, or by Mr. Cock's where the urethra is impermeable.

We could have prolonged almost indefinitely our comparison of the works of these representative teachers. Looking at the books as a whole, we would say Mr. Bryant's is the more full, systematic, and complete ; Mr. Spence's far the most interesting to read and instructive to the surgeon. Neither are easy books

for a student; Bryant's is too long, and does not sufficiently distinguish what he *must* know, from what he need not load his memory with. Spence's is awkwardly arranged, and does not attempt even to facilitate study. Both are well suited for a practical surgeon, and Spence's especially will in some departments have the authority of a master. In hernia, tracheotomy, and injuries of the head, Mr. Spence will be a classical authority long after he has ceased to teach from the chair of surgery in Edinburgh.

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#### IV. The Sickness and Mortality in the Army and Navy.<sup>1</sup>

THE delay in the appearance of the Army Reports for 1873 and 1874 has prevented us from presenting to our readers an outline of the chief features connected with the health of the army and navy for three years so early as we desired. It may be in the recollection of many that Dr. Balfour, who had prepared the statistical reports for the army for so many years, left the medical department at Whitehall on promotion, and was succeeded by Deputy Inspector-General Barclay, who compiled the returns for 1872. On that gentleman's death, late in 1874, Sir A. Home, K.C.B., was appointed to the office, and the reports which form the subject of this notice were prepared under his supervision. The Naval Reports continue to be drawn up by Dr. Mackay, Deputy Inspector-General of Fleets and Hospitals, as previously.

The Army Report for 1874 contrasts with those which preceded it in the absence of the appendices containing the details of the diseases causing the sickness, mortality, and invaliding; the bulk of the volume is reduced in consequence to less than half that of those for 1872 and previous years, but its usefulness as a statistical record is thereby greatly diminished; for although the results be given in the text in a condensed form under the different orders, still, as these embrace many complaints of varying importance (the order of diseases of the digestive system, for instance, including dysentery, diarrhœa,

<sup>1</sup> 1. *Army Medical Department Report for the Year 1873.* London, 1875, p. 509.

2. *Army Medical Department Report for the Year 1874.* London, 1876, p. 220.

3. *Statistical Report on the Health of the Navy for the Year 1873.* London, 1874, p. 596.

4. *Statistical Report on the Health of the Navy for the Year 1874.* London, 1875, p. 586.

and the various affections of the liver, which cause a large mortality, besides many others of lesser consequence), it is impossible to separate the more serious diseases from the less serious, or from each other, and thus those comparisons of the prevalence of particular diseases at different periods, and under varying circumstances, so essential for elucidating their causes, and devising means for their prevention, will cease to be possible for inquirers who have not access to the records of the statistical branch of the office, and this want of access must of necessity put it out of the power of by far the greater number of medical officers to engage in such investigations. Hitherto all the statistical reports on the health of the army, from the earliest on the sickness and mortality among the troops in the West Indies, issued in 1836, have contained full details of the individual diseases, and their publication has enabled many persons to study their causes with a degree of success which has contributed largely to the improvement in the health and efficiency of the soldier. It would be a subject for serious regret if this were rendered impossible hereafter, and we trust the authorities may see their way, not only to supplying these details in future reports, but, to prevent any break in the records, to including those for 1874 in the report for 1875.

A fair idea of the health of the army may be obtained from the following table, which shows the ratio of deaths per 1000 of strength among the white troops, and the numbers per 1000 constantly sick, at the chief stations for the years 1873, and 1874; the mean ratios for the ten years 1863-72 are added for comparison :

STATIONS.	Died per 1000.			Continually Sick per 1000.		
	1873.	1874.	1863 to 1872.	1873.	1874.	1863 to 1872.
United Kingdom . . . . .	8.26	8.79	9.25	37.47	38.59	42.64
Gibraltar and Malta . . . . .	7.36	7.27	11.00	34.35	35.25	38.69
Dominion of Canada . . . . .	11.19	6.00	9.40	25.48	29.68	30.83
Bermuda . . . . .	8.50	10.18	27.65	30.21	27.94	39.05
West Indies . . . . .	12.80	16.90	16.41	43.72	46.55	46.62
St. Helena and Cape . . . . .	6.13	14.40	10.86	47.45	43.61	51.15
Mauritius . . . . .	27.20	16.74	16.75	44.35	35.83	50.41
Ceylon . . . . .	12.32	6.04	23.71	41.60	36.67	60.98
China and Straits Settlements . . . . .	15.07	9.77	42.28	53.84	43.81	68.99
India . . . . .	16.25	14.22	24.96	54.41	55.69	58.38

If the eye be run over this table it will be perceived that in the Mediterranean stations the mortality in 1873 and 1874

was practically the same in amount, while in both it was considerably less than the mean of the previous ten years.

In this country, Bermuda, the West Indies, and the Cape, the mortality was greater in 1874 than in 1873; in the first two it was still less than the average of the previous ten years, in the West Indies it was slightly and at the Cape considerably greater. On the last-named station there was an increased number of deaths from every class of disease (including accidents) causing them in 1873; those from aneurism amounted to 2·4 per 1000; suicide alone showed no increase. In the Dominion of Canada, Mauritius, Ceylon, China and the Straits Settlements, and India, the mortality in 1874 was less than in 1873, and in the last three was, in both years, much under the average for the period 1863-72. The force in Mauritius being now but little over 400 men, considerable fluctuations in its rate of mortality must be looked for. The columns giving the average numbers under treatment correspond in the main with those for the deaths, though they do not follow them closely, and they show that on most stations there has been a sensible reduction in the loss of time by sickness in the two years under consideration, in comparison with the preceding ten.

It may be supposed that the greater facilities for invaliding latterly may have enabled men to be sent home before their cases became so aggravated as to prove fatal on the station. The ratios given above, however, include the deaths among the invalids from each station, not only on their passage home, but until their disposal after their arrival in this country, and the following table, showing the millesimal ratios of invalids sent home for 1873 and 1874, and for the period 1863-72, will permit of a similar comparison to that instituted above in connection with the deaths and numbers constantly sick:

STATIONS.	1873.	1874.	1863 to 1873.
United Kingdom . . . . .	25·26	29·66	29·02
Gibraltar and Malta . . . . .	27·70	26·11	26·46
Dominion of Canada . . . . .	23·56	23·42	16·29
Bermuda . . . . .	19·50	21·97	23·13
West Indies . . . . .	17·08	28·02	32·30
St. Helena and Cape . . . . .	21·25	18·10	29·28
Mauritius . . . . .	22·68	19·13	46·65
Ceylon . . . . .	58·53	34·20	45·65
China and Straits Settlements .	30·32	48·29	74·71
India . . . . .	39·13	40·19	41·03

The ratios here for the United Kingdom indicate the number actually discharged from the service on account of inefficiency,

the result of disease; while those for the foreign stations include all who were sent home on this account, many of whom recovered sufficiently during the passage to be able to resume their duties on their arrival in this country or soon after, and a much smaller number only were discharged from the service. In the United Kingdom the invaliding in 1874 was slightly above that from 1863 to 1872, but the mean of 1873 and 1874 is less. Similarly in the Mediterranean stations the mean of the last two years is fractionally less than that for the previous ten. The proportion of invalids sent from Canada in 1873-4 was nearly a half greater than in the preceding two years, and the ratio for Ceylon in 1873 was much higher, either than in 1874, or from 1863 to 1872; in these cases, however, the numbers per 1000 actually discharged from the service were—

	1873.	1874.	1863 to 1872.
Dominion of Canada . . . .	13.54 . .	16.22 . .	12.90
Ceylon . . . . .	22.59 . .	9.05 . .	22.38

So that a large proportion of slight cases must have been invalided, and being from small bodies of troops the fluctuations appear larger than would have occurred had the numbers under observation been greater, or the period more extended. In all the other stations the numbers invalided in 1873 and 1874 were smaller than in the previous ten years, clearly indicating that the lessened mortality and improved health shown in the first table were not dependent on the greater facilities now existing for sending men home from foreign stations. During 1873 and 1874 there was an almost complete immunity from those severe epidemics of fever formerly not infrequent, and cholera was remarkably quiescent all over India; something of the favorable results, no doubt, is owing to these circumstances, but enough remains to prove that the increased attention to the hygienic management of the troops, even since 1863, has borne good fruit.

The reports under consideration afford evidence that enteric fever has been attracting more attention than formerly, and there are accounts of its having been met with in small numbers in both years on nearly every foreign station; at the same time, from the high ratio the deaths bear to the cases admitted to hospital, it is clear that no inconsiderable portion escapes recognition altogether. Where periodic fevers are common there is, no doubt, often much difficulty in distinguishing cases of enteric fever at their commencement, for they are then apt to assume a periodic form, and unless they prove fatal, and the enteric lesion be verified by post-mortem examination, they may be classed as intermittents or remittents, and their true character

be altogether lost sight of. In 1873, on the home station, 128 cases were returned, of which thirty proved fatal; in 1874 ninety-two cases were diagnosed, of which eighteen proved fatal; in the large standing camps, which in both years contained about 21,000 men, there were in 1873 eight cases only with two deaths, and in 1874 seven cases with three deaths. In 1873 the chief local outbreak was at Kinsale, where there were twenty-two admissions from January to March; in Dublin there were eighteen cases, which were received from ten different regiments, occupying five different barracks placed widely apart; at Dartmoor camp of exercise twenty cases occurred, thirteen of them in the 19th Hussars, and the remainder in six other regiments; there were, besides, smaller outbreaks of seven at Hounslow, six at Fleetwood, and five at Enniskillen. In 1874 there were outbreaks at Newbridge of eighteen cases, commencing in January; at the military prison at Dublin of fourteen cases in August; and in that at Limerick in November. The two former were attributed to sewer gases, and the last to water contaminated with surface drainage. Altogether, in 1874, admissions from enteric fever were reported from twenty-six stations, from eight of which one case each was returned, from eleven two cases each, so that while the cause was extensively diffused the effects at most points were but very limited.

In the end of 1873 there were indications of enteric fever at the usually healthy mountain station of Newcastle, in Jamaica, and in 1874 there were twenty-six admissions from this disease recorded, of which fifteen proved fatal. In recent years enteric fever was rarely seen among the troops in Jamaica, though circumscribed outbreaks were occasionally met with of considerable severity; but as a general rule there was no trace of the enteric lesion in the intestine in the fatal cases of fever at that station. What should have led to such an unusual prevalence of it at Newcastle, the details at the disposal of the compiler of the reports are insufficient to show satisfactorily.

In India enteric fever continues to receive increased attention. The admissions and deaths in the three Presidencies were in—

		1873.		1874.	
		Admitted.	Died.	Admitted.	Died.
Bengal	.	143	53	168	71
Madras	.	52	10	34	12
Bombay	.	17	13	88	15
		<hr/>	<hr/>	<hr/>	<hr/>
		212	76	235	96

The high proportion of fatal cases here, as elsewhere, seems to indicate that many are still over-looked, and classed under other diseases, and it is obvious that, until the cases be more generally recognised at their commencement, but little progress can

be made in connecting them with the particular circumstances under which they originate. So far, young men recently arrived in the country seem to suffer most, and though the disease be widely diffused, it was intensified at certain stations only in each year, and in some instances where more corps than one occupied the same station, one or other of these contributed a much larger proportion of cases than the remainder. Without full details as to the numbers at each age in these corps, and those serving with them, their length of residence in the country, and particulars as to their exposure to the various supposed sources of the disease, it would evidently be premature to attempt to generalise on the foregoing bare statistical facts.

In 1874 the return of primary venereal sores and gonorrhœa, which had previously been given for the fourteen stations never under the Contagious Diseases Acts, and for the fourteen that are now under that of 1869, for each year from 1864 to 1872, were extended so as to embrace the period from 1860 to 1873. An abstract of these showing the prevalence of these diseases in each of the groups above mentioned by years, was also given, and these were arranged in three periods, viz. from 1860 to 1863, before any restrictive measure was in force; from 1864 to 1869, when the Act of 1864, followed by those of 1866 and 1869, were gradually extended to all the fourteen stations now under the last Act; and from 1870 to 1873, when that Act was in full operation at these fourteen stations. We cannot afford space to transfer the entire return to our pages, but the averages for the periods mentioned will convey to our readers the prominent facts of the case, and enable those who have not had the opportunity of perusing the original to appreciate the influence of the Act.

PERIODS.	Stations never under Acts.			Stations ultimately brought under Acts.		
	Average strength.	Admitted per 1000.		Average strength.	Admitted per 1000.	
		Primary venereal sores.	Gonorrhœa.		Primary venereal sores.	Gonorrhœa.
Average 1860 to 1863	16100	116	129	49387	180	135
" 1864 „ 1869	16899	108	119	41295	87	120
" 1870 „ 1873	19390	108	101	48627	52	101

Here it will be perceived that, at the stations where no restrictive measures were employed, the average rate of admissions per 1000 in the first period was, for primary sores, 116; and that in the last period these had fallen to 108, while, at the stations which ultimately came under the Acts, the ratio in



the first period, though considerably higher, viz. 130, diminished to 87 in the second when the Acts were partially in force, and to 52 in the last when they were in full operation. It has been objected to the table from which the above averages are taken that, as London is excluded up to 1866, while it is included with the other stations never under the Acts from 1867 to 1873, the comparison between the first and second periods and the third is vitiated: the reason for exclusion was that the admissions at Windsor, one of the stations which subsequently came under the Acts, could not be separated from those at London before 1867. As London, however, is the only place in the vicinity of the large stations where the Act is in force, where any considerable body of troops is collected, we think it should not be omitted, and the fact that the admissions at Windsor are included forms no valid objection to this, for the strength there was only about one seventh of the aggregate force at both stations, and from all we have been able to learn, up to 1866, the prevalence of venereal complaints was much about the same as at London, so that the ratios at the latter will not be sensibly affected thereby. That our readers may see how far, and in what direction the association of London, up to 1866, with the other thirteen stations which were under the Acts affects the question, we subjoin the strength and ratios per 1000 of admissions for primary venereal sores, and gonorrhœa, calculated on this basis, for comparison with those given above.

	Average Strength.	Ratios per 1000 admitted.	
		Prim. Venl. Sores.	Gonorrhœa.
1860—63 . . . .	21,398	122	112
1864—69 . . . .	19,520	114	112
1870—73 . . . .	19,090	108	101

These figures show higher ratios for primary sores in the first two periods than when London was not included, but lower ratios for gonorrhœa. As these compare with the last period in which London was embraced in the original table, the view they afford is more trustworthy, and should be adopted in preference to that afforded by that table. If we consider that the ratios of admissions for primary venereal sores and gonorrhœa at the stations now under the Acts, indicate the prevalence of the causes of these affections over the country, their relative activity in the two periods 1860-63 and 1870-73 would be, for primary sores as 122 to 108, showing a reduction of 11·5 per cent., while at the stations which came under the Acts the fall was from 130 to 52 or 60·0 per cent., more than five times greater. So with gonorrhœa, at the stations now under the Acts the fall from 112 to 101 amounts to 9·8 per cent., as against one from 135 to 101, or of 25·2 per cent. at the stations where the Act was in force.

In the Report for 1873 a table is given, in continuation of former returns to the same effect, showing the number in hospital daily with primary venereal sores at the stations now under the Act, and those which were under it from 1870 to 1873. From this it appears that, on the average of the four years, at the former 9·48 men per 1000 were constantly sick with the primary form of the disease, whilst at the latter there were only 4·33. Such are the practical results of the working of the Contagious Diseases Acts on the health of the troops in this country; further on we shall notice their effect, in recent years, on that of the navy.

The Appendix to the volume for 1873 contains a "Report on Hygiene for 1874 and part of 1875," by the late lamented Dr. Parkes, the last contribution he made to these records, his health having given way so far that he was unable to draw up another for the following period. The great value of these Reports of Dr. Parkes will be readily admitted by all who have perused them, and whether the reader agree with the writer's views or not, he cannot but admire the watchful care he displays in giving every portion of evidence on the question within his reach with the most perfect impartiality.

There is an extensive series of papers in connection with the Ashanti expedition in the same volume; these embrace:—  
1st. The preliminary memoranda and instructions prepared in this country, and the arrangements connected with the expedition.  
2nd. A medical history of the war, by Sir Anthony Home, V.Cf, from its commencement to the end of December, when he had to resign medical charge in consequence of severe illness, and was invalided to England; this was subsequently brought up to the end of the war.  
3rd. A description of the fittings of the Victor Emmanuel Hospital Ship, and the details of the medical establishment, and stores on board, and the instructions issued for conducting the duties.  
4th. Short papers on the water at Cape Coast Castle, and on some of the fatal cases, by Dr. Fleming, Pathologist to the Expedition. Space does not permit us to do more than merely allude to these papers, but we can assure such of our readers as desire detailed information on the points they embrace, they will find their perusal most interesting. The expedition was forcibly described at the time as a "Doctor's War," and fortunately, not only for those immediately engaged, but for the country, the advice of the doctors was taken not to commence operations by the white troops till towards the end of December, though great pressure had been brought to bear on the authorities at the War Office, through the daily press and otherwise, to begin much earlier. As it was the three European regiments, that were on shore

about two months, had an admission rate of 586 per 1000 of their strength from disease (chiefly fevers), though with but a moderate mortality; but had they advanced from Cape Coast early in November, or even at the commencement of December, much more sickness, and of a more serious description, would have been encountered, the force would have been paralysed like the marines a few months before, and had it ventured beyond the Prah the expedition could only have resulted in a deplorable disaster.

The appendix to the volume for 1873 contains several other papers of considerable merit, which we cannot enter upon on this occasion. The volume for 1874, as already mentioned, is much less in bulk than that for 1873, owing to the omission of the detailed returns of diseases for the different stations, and also to the fact that the number of contributions in the appendix is much more restricted. These embrace a paper on the Medico-Military Topography of the Persian Gulf and Valley of the Euphrates and Tigris, by Dr. G. H. Evatt, Army Medical Department, and notices of the more important surgical cases at Netley, by Surgeon-Major Porter, besides other shorter papers of less importance.

We now turn to the Statistical Reports for the Navy, and shall present our readers with abstracts of the sickness, mortality, and invaliding on the different stations, as nearly as possible in the same form as those already submitted for the army, embracing the years 1873 and 1874, and the period 1863-72 for comparison. The naval returns differ from those of the army in giving the deaths on the station only, and in not including those among invalids on their way home. They also embrace deaths from drowning, to which seamen are, of course, much more exposed than troops, and the loss from this cause is very variable in amount; the average from 1863 to 1872 for the Home Station is thus materially increased by the loss of the "Captain" in 1870, and that for the South-east coast of South America by the burning of the Bombay in 1864. In the two years under review the mortality from drowning fluctuated greatly; thus, in China, in 1873, it amounted to 1·9 per 1000 of strength, and in 1874 to 5·2 per 1000; and, on the average of the ten-year period from 1863 to 1872, it varied from 1·0 per 1000 on the Mediterranean Station, to 12·6 on the South-east coast of South America. It is obvious this cause of disturbance of the death rate must be eliminated before the effect of disease can be fairly appreciated, or the ratios for the different years, or stations, compared with each other. In the following table this has been done, and it presents the ratio per 1000 of deaths from disease and

ordinary accidents, for the years 1873 and 1874, and the average for the ten years 1863-72; also the number per 1000 of sick under treatment daily, both on board and in hospital, in these years, on the stations named.

STATIONS.	Died per 1000.			Constantly Sick per 1000.		
	1873.	1874.	1863 to 1872.	1873.	1874.	1863 to 1872.
Home . . . . .	5.5	6.2	6.2	41.8	42.8	41.5
Mediterranean . . . . .	4.8	7.3	6.8	52.7	54.6	48.4
North America and West Indies . . . . .	16.4	7.6	10.7	44.1	51.0	50.7
S.E. Coast of South America . . . . .	4.8	32.4	10.6	25.0	37.0	46.6
Pacific . . . . .	5.2	3.2	6.7	47.6	40.3	56.2
W. Coast of Africa and Cape of Good Hope . . . . .	25.0	21.9	18.9	58.3	59.1	65.7
East Indies . . . . .	11.1	10.2	12.7	46.8	48.3	51.5
China . . . . .	6.7	7.1	14.4	49.4	64.5	72.6
Australia . . . . .	1.9	7.3	8.4	48.3	49.1	47.2

None of these stations are fairly comparable with the corresponding ones for the army, except the Home Station, and in a lesser degree the Mediterranean, and more remotely still China. The North American station of the navy really embraces the Dominion of Canada, Bermuda,\* and the West Indies of the army; the West Coast of Africa and Cape Station of the former embraces the tropical African stations, St. Helena, and the Cape, of the army; and the East Indian station of the navy includes India, Ceylon, and Mauritius. The advantages to the navy with regard to health of these more extended stations are very great, for, when a ship has begun to suffer at the more sickly ports of the command, she can be, and in practice very commonly is, sent to a more salubrious locality, where her crew seldom fail to shake off the evil effects of their previous exposure. Partly from the deaths of invalids not being included, partly also from the men being generally engaged for shorter periods, the mortality in the navy is materially less than that in the army; but, after making full allowance for these, it would appear that constant residence on shore, and exposure to the local sources of disease which that involves, has led hitherto to a rate of mortality among the troops permanently higher than among the seamen of the navy. Thus, while the ratio of deaths among the troops in the United Kingdom in the ten years 1863-72 was 9.25 per 1000, that in the navy was 6.2 only: in the Mediterranean the ratios for the same period, were 11.0 and 6.8 respectively; and, in the

stations to the eastward of the Cape, the difference in favour of the navy was much greater.

If the death rate in 1873 be compared with that in 1874, it is found that, on the Home Station, there was an increase in the latter year, as was the case in the army, and it is worthy of observation, that this was accompanied by an increased mortality among the civil population in both England and Scotland, in 1874, of 1·2 per 1000 in the former, and of 0·9 per 1000 in the latter, over 1873. These relations cannot be accidental, but we cannot do more than allude to them on this occasion. In the Mediterranean, China, and Australia, the mortality was greater in 1874 than the previous year; on the South-east coast of South America it was greatly increased owing to the prevalence of fever at Rio Janeiro. On the other hand there were slight reductions on the Pacific, and East Indian stations, and a much larger one on the North American and West Indian command, in consequence of an outbreak of yellow fever at Port Royal, in Jamaica, in 1873, ceasing at the end of that year, and the almost complete immunity of the ships in 1874 from that disease. The excess of mortality on the West Coast of Africa, in both years, was altogether due to the exposure of the marines and seamen on shore during the operations connected with the Ashanti invasion. These large fluctuations prevent such a comparison between the two years and the ten-yearly period, for several of the stations, as was made with the army; but, notwithstanding the low rates attained during the latter, 1873 and 1874 still showed favourably with them on the Mediterranean, Pacific, and Australian stations.

That part of the table which gives the number of men on the sick list daily, brings out a feature familiar to those who have had experience, especially of tropical diseases, viz., that high mortality is by no means inseparably connected with large sick lists; yellow fever, or cholera, are very fatal, though of short duration, while intermittents, and diarrhœa, cause fewer deaths, but are very difficult to shake off, and consequently involve much longer hospital treatment; this feature is well illustrated by the instances of the North American and West Coast of Africa commands, where with the smaller mortality, the numbers constantly sick were actually higher than with the greater. As in the army, however, there is evidently a reduction of the sick time in the last two years over the previous ten, on the South-east Coast of America, Pacific, West Coast of Africa and Cape, East Indian, and China stations, indicating a still improving condition of the health and efficiency of the men in those important commands.

The following table gives the ratios per 1000 of men in-

validated at the various stations, in 1873 and 1874, with those for the previous ten years, to complete the general view of the health of the navy.

STATIONS.	1873.	1874.	1863 to 1872.
Home . . . . .	35·6	29·9	28·1
Mediterranean . . . . .	36·4	34·5	36·7
North America and West Indies . . . . .	30·4	34·5	30·6
S.E. Coast of South America . . . . .	15·8	54·0	26·4
Pacific . . . . .	22·3	34·9	30·8
W. Coast of Africa and Cape . . . . .	119·8	193·4	73·8
East Indies . . . . .	34·4	59·1	54·1
China . . . . .	46·6	31·8	55·7
Australia . . . . .	13·5	20·7	21·1

The numbers here are more irregular than they were in the corresponding table for the army. While the mean of 1873-4 shows a reduction on that for 1863-72, on the Pacific, East Indian, China, and Australian stations, there is a great increase on the West Coast of Africa; but the Home, Mediterranean, and North American stations, are but little altered. As compared with the army the most prominent feature is the higher ratio on foreign stations, with the single exception of China, thus, while the invaliding from the troops at Malta and Gibraltar, from 1863 to 1872, amounted to 26·46 per 1000, that from the squadron in the Mediterranean for the same period was 36·7, and while the troops, in the same period, invalided from Mauritius 46·65, from Ceylon 45·65, and from India 41·03, the squadron on the East India station sent home 54·1. The unsuitableness of ships for the treatment of many forms of disease on the one hand, and the necessity for keeping their crews up to their establishment on the other, no doubt lead to this great difference in the practice of the two services.

In 1873 there was a considerable outbreak of yellow fever, almost confined to the Aboukir, receiving ship at Port Royal, Jamaica, and to a certain part of the dockyard there, in the vicinity of a drain that was offensive. The Aboukir had been at Port Royal as receiving ship since 1862, and, like most of her predecessors in that capacity,—not only in Jamaica, but in other parts of the West Indies, and at Sierra Leone,—at length became the locality of a severe outbreak. These attacks have generally occurred during an epidemic season, and the sanitary condition of the ship, as was the case with the Aboukir on this occasion, was bad. Under such circumstances yellow fever may spring up, and prevail with great intensity, in a circum-

scribed locality, while the people in ships or houses in the vicinity may remain exempt, as the few white troops at Port Royal are reported to have done on this occasion. In 1873 the receiving ship at Rio Janeiro, the *Egmont*, which had been there in the same capacity since 1863, had a considerable number of cases of fever on board, returned as yellow; in 1874 this ship was again very sickly, and she was at last ordered to be cleared of her stores, and sold; when empty, it was found that a very foul condition existed in the after part of the hold, and it is said five deaths took place among those most exposed to the emanations from this source. There are now many instances on record of ships having conveyed the cause of yellow fever in their bilges, not only within the tropics, but into temperate countries, and from the West Indies to Europe; persons exposed to the emanations from these holds have been attacked, both in this country and France, while mere communication with the cases led to no disease. We believe that if writers on yellow fever were to keep this fact in view, and to endeavour to eliminate the possibility of the operation of a local cause in the vessel, or locality, before concluding that the disease spread only by contagion, much of the present confusion of opinion regarding the causes of the disease would disappear.

Enteric fever was frequently met with in 1873, there having been 99 cases returned, and 24 deaths; in 1874 there were 46 admissions and 20 deaths. In the former year the South-east coast of America was the only station where one or more cases did not occur, and in the latter the same station, with the Pacific, and West Coast of Africa, and the Cape, were exempt; the high death rate in proportion to the number of cases returned suggests, as in the army, that many attacks must have escaped recognition, and be included under other designations. This difficulty of distinguishing remittent from enteric fever at the commencement is particularly noticed by Staff-Surgeon Haran of the *Glasgow*, at Bombay, and he advances the view, which it is difficult for a close observer of the two diseases in the tropics to resist, that the poisons of both the remittent and enteric fevers may be in the system at the same time, the remittent being the prominent disease at first, and, as that gives way, the enteric takes its place as its peculiar lesions become developed. There is one striking feature in many of the cases recorded in the Naval Reports, namely, the occurrence of inflammation of the fauces and tonsils, and even of considerable portions of the lungs, as the apparent affection at the commencement, and which ultimately passes on to the fully developed enteric fever.

In most instances the cases occurred singly or in small numbers, at considerable intervals, in different ships, and generally the sufferers had had communication with the shore within the usual incubation periods, so that they afford no precise information on disputed points as to the causes of the disease. In two instances, in 1873, this fever followed the use of water obtained at Vigo. In the first, the flying squadron, consisting of the five frigates, *Narcissus*, *Doris*, *Aurora*, *Endymion*, and *Topaze*, met there in the end of January, and sailed for the West Indies on 5th February. All five took in water at Vigo, but it does not appear whether it all came from the same source; that received by the *Doris* is said to have shown, under the microscope, in addition to vegetable matter, numerous living animalcules, and some striped muscular fibre. At sea enteric fever commenced in the *Narcissus* on 15th February, and in the *Doris* on 20th. Up to 7th March there were fourteen cases in the *Narcissus* and sixteen in the *Doris*, with twelve others classed as continued fever. In the other three ships no fever seems to have shown itself. In the other instance the *Minotaur* and *Sultan* had lain at Vigo from 1st March to 11th April, when they left for England, and anchored at Spithead on 15th; the former sent nineteen cases of enteric fever to Haslar Hospital and the latter sixteen, and others on subsequent days, until they amounted to twenty-five for the *Minotaur* and seven-teen for the *Sultan*. These ships took in water at Vigo, and several of the men who suffered had not been on shore there. There seem to be three sources from which water is obtained at Vigo; specimens of each were sent from the *Minotaur* for analysis by Professor Frankland, who stated they contained a large portion of organic matter of vegetable origin, which he concluded could not affect their wholesomeness; on the other hand there is the report from the *Doris*, that the supply she obtained (it is presumed from one of these sources) contained muscular fibre, which would indicate the possibility of sewage contamination.

There were two instances of enteric fever appearing in 1874 which deserve notice. In one, in the *Agincourt*, a well-conducted young man of twenty-one went on the sick list with this disease on 26th January at Lisbon; he had not been out of the ship since she left England in October, 1873. He was employed in the paint room, and from time to time complained of a disagreeable odour there, to remove which a solution of carbolic acid had been employed on several occasions. On examination two brown-coloured stains were found on either side, corresponding with the position of the junior officers' water closets in the bow of the ship on the main deck. It was sup-



posed the man contracted his illness from exposure to this leakage. Measures were taken to prevent leakage here in future, and no other case occurred. The other instance was in the Malabar troop-ship, on her passage from Bombay to England. She left Bombay on 25th November, and between Aden and Suez the latrines used by the troops and seamen became exceedingly offensive, owing to some defects in the pipes which prevented their being flushed sufficiently often, and allowed a fæcal accumulation to decompose under the influence of a hot sun with exceeding rapidity. On 11th December, while passing through the canal, a young stoker reported himself sick with what proved to be enteric fever; this man had been on shore last at Bombay on 22nd November for about two hours, returning to the ship about sunset. Two artillerymen, passengers, were attacked subsequently to the stoker with enteric fever. A thorough rectification of the pipes took place, followed by flushings of the latrines, and there were no more cases of disease. The water used on board was condensed, and was of excellent quality. Looking at all the circumstances, the medical officer, Staff-Surgeon W. Roche, concluded the cases originated from exposure to the emanations from the latrines; the period which had elapsed since the men were last exposed at Bombay may have scarcely been long enough to exclude the possibility of their having imbibed the poison there so completely as desirable, but the opinion of Mr. Roche seems very probable, and receives support from the instance in the Agincourt.

In the report for 1874 the progress of primary and secondary syphilis in the Home force is given year by year, from 1861 to 1874 inclusive. Up to 1865 the secondary cases were included with the primary in the returns, but from 1866 onwards these have been separated. During the nine years 1866-74 there were 11,184 cases of primary sores and secondary syphilis, of which 2976 were of the latter form, constituting 26·6 per cent. of the whole, and as the ratio of the secondary disease to the primary sores varies within very narrow limits, no material error can arise if the sums of the two forms from 1861 to 1865 be separated in the proportion just indicated. Carrying out this process, and arranging the numbers for the periods 1861-63, 1864-69, and 1870-73, we obtain the following results, which may be compared with those for the troops in the United Kingdom already given:

Period.	Home Force.	Primary Sores. Ratio per 1000.	Secondary Syphilis. Ratio per 1000.
1861-63 . . . . .	21,743 . . . . .	76·5 . . . . .	27·7 . . . . .
1864-69 . . . . .	21,452 . . . . .	52·6 . . . . .	18·3 . . . . .
1870-73 . . . . .	22,125 . . . . .	39·6 . . . . .	15·1 . . . . .

The decrease of the primary sores during the second period,

when the Acts were becoming more stringently enforced, and extended to various outposts not at first included, is very decided and the contrast of the last period, when the Act of 1869 was in full operation, with the first, when no repressive measures were in use, is equally convincing as in the case of the troops at the stations under the Acts, as to the advantage which has been derived from its operation in limiting the spread of these diseases. The fall in the ratio per 1000 of secondary syphilis as that of the primary forms diminishes corresponds with what is found in the army, and proves that the measures taken under the Act lessen the frequency of the so-called non-infecting sores in the same proportion as the infecting sores.

In addition to the details of the sickness on board ship, the appendix to each of the volumes for 1873 and 1874 contains the reports of the medical officers in charge of the various divisions of marines, of the dockyards, and of the naval hospitals, in this country; also papers on the physical examination of boys for the navy and of recruits for the marines enlisted in London, and notes and statistics relating to the boys under training for the navy. These all embrace matter of interest, which our want of space prevents us doing more than allude to, and for which we must refer our readers to the reports themselves.

### V.—Recent Ophthalmological Studies.<sup>1</sup>

THAT the refraction of the eye could be determined by means of the ophthalmoscope was already mentioned by Helmholtz, in 1851, in the pamphlet in which he described his instrument. He quotes a case<sup>2</sup> in which he was able to decide by examination of a perfectly blind eye, whether certain visual troubles of old date had been attributable to short-sight or to commencing amblyopia, a question of importance. He states that the existence and *degree* of short or long sight can be thus easily determined, and urges that the method is of importance, because

<sup>1</sup> 1. *Determination of the Refraction of the Eye, by Means of the Ophthalmoscope.* By EDWARD C. LORING. New York, 1876.

2. *Zur Lehre von den Ursachen der Kurzsichtigkeit.* Von Dr. J. SCHNABEL, ('Archiv für Ophthalmologie,' xx, 2, p. 1).

3. *Can Staphyloma Posticum be induced by Astigmatism?* By W. THOMSON. ('Trans. of the American Ophthalmological Society,' Eleventh Annual Meeting, July, 1875, p. 310).

4. *On the Connection between Staphyloma Posticum and Astigmatism.* By W. THOMSON, ('Amer. Journ. of Med. Sciences,' new series, vol. 70, p. 383, Philadelphia, 1875).

<sup>2</sup> 'Beschreibung eines Augenspiegels,' p. 38.

it renders the observer independent of the patient's assertions. Schweigger quotes the statement, and adds, that the cases are not uncommon, in which it is only by means of the ophthalmoscope that the way is found for a satisfactory functional examination.

Jäger's paper on the same subject in the 'Austrian Journal of Practical Medicine' a few years later, seems to have been little noticed, though he asserts that such accuracy can be easily attained in the choice of glasses by this method that the utmost error does not exceed one or two of the successive numbers in ordinary use for spectacles.

The great work of Donders on the anomalies of accommodation and refraction of the eye, for the publication of which we are so much indebted to the New Sydenham Society, contains a short but clear exposition of the method. He considers that in general—

"This method is inferior in accuracy to the determination of vision with glasses of known focal distance. 1. It is for many observers difficult, in the use of the ophthalmoscope, entirely to relax their power of accommodation: if they are not certain of this, the method is inapplicable to them. He who, on the contrary, has by practice attained so far that he can not only wholly relax his power of accommodation, but also justly estimate the degree of voluntary action, can very often usefully employ it. I know this by my own experience.

"2. Without producing paralysis of accommodation, we are never perfectly sure that we determine the refraction in the condition of rest.

"3. It is sometimes difficult, at least when strongly negative glasses are required, with a narrow pupil accurately to see the vessels of the retina.

"4. The vessels which lie at different depths in the fibrous layer afford no perfectly correctly situated object for estimation.

"5. Moreover, such a vessel is not a suitable object to determine with precision whether we see accurately. Consequently the method in each case requires a great degree of attention.

"6. The determination in the line of vision, which it chiefly concerns, is for the most part difficult of execution, because the place of the yellow spot is not well seen, or our estimation of the accuracy of seeing it is particularly difficult."

Notwithstanding these doubts and difficulties, the method deserves attention, because it is applicable where functional examination fails, as in young children or in the blind. More-

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<sup>1</sup> "Ueber die Verwendung des Augenspiegels als Optometer," 'Oesterreichische Zeitschr. für prakt. Heilkunde,' 1856, and repeated in his treatise 'Ueber die Einstellungen des dioptrischen Apparates im menschlichen Auge.' Wien, 1861, p. 1.

over, the refraction in regard to the periphery of the retina can be tested in this way: Donders says:

"In many instances I have by it alone succeeded in satisfying myself that the myopia for indirect vision was less than when the patient looked in the line of vision.<sup>1</sup> Besides, the want of fixation of a hypermetropic eye examined with the ophthalmoscope, sometimes gives rise to more complete relaxation of the power of accommodation, whereby hypermetropia, latent in trials of vision, may manifest itself."

The subject was treated some years later by Mauthner at great length in the sixth chapter of his very good book on Ophthalmoscopy. The chapter was also published as a separate essay. He thinks so highly of the method by the erect image, that he gives (p. 175) the general rule:—In all cases when the patient complains about troubles possibly arising from abnormal refraction, the surgeon should use the ophthalmoscope before having recourse to print and glasses. In this way, provided he has the necessary skill, he will learn not only the existence of any abnormal refraction, but also its exact degree. Subsequent tests by glasses may confirm the diagnosis. Should they, however, as is often the case in H, give a different amount, that

<sup>1</sup> Dr. W. Stammeshaus has published some interesting researches on the refraction of the eye, in respect to the periphery of the retina (Ueber die Lage der Netzhautschale zur Brennfläche des dioptrischen Systems in the 'Archiv für Ophthalm.,' xx, 2, p. 147). He has found with the ophthalmoscope that, in emmetropic eyes, the refraction becomes hypermetropic about 4 mm. behind the equator, or about five times the diameter of the disc measured from the inner side of the disc itself. The change is sometimes gradual, but more often rather sudden. He has found at the spot mentioned a hypermetropia of about  $\frac{1}{10}$ , and thinks himself safe in asserting that it is never less than  $\frac{1}{10}$ . There are, therefore, two zones in the retina: an inner one, surrounding the macula lutea, with emmetropic refraction; and a second, forming an outer ring where there is marked hypermetropia.

In cases of great central hypermetropia,  $\frac{1}{4}$  or more, the author has found scarcely any increase at the periphery. This would support the view that the hypermetropic eye is shortened principally in its sagittal diameter.

The peripheral refraction varies considerably in myopia. The author relates two cases which probably represent the extremes, between which the refraction at the periphery of the retina may vary in comparison with that at the centre. The one patient, twenty-four years old, has in each eye M  $\frac{1}{2}$ , with insufficiency of the internal recti muscles; has used for years in his work -12; S oc. dextr.

with  $-6 = \frac{15}{20 \text{ to } 15}$ . The refraction is unchanged over a space of several disc-

diameters from the inner edge of P; then comes a place where there is a M of  $\frac{1}{4}$ . The difference  $\frac{1}{2} - \frac{1}{4} = \frac{1}{4}$  is somewhat greater than in the emmetropic eye. Assuming the absence of any change in the refracting media, the peripheral just as the central M must be due to an elongation of the corresponding diameter. The second patient, twenty-six years old, has for years constantly used -8. His M =  $\frac{1}{2}$ , S =  $\frac{15}{20 \text{ to } 15}$ . At the periphery, just as in the emmetropic eye, there is great H (about  $\frac{1}{4}$ ). Thus, the eyeball is elongated only in the direction of its axis.

will not in the least diminish the reliability of the ophthalmoscopic result.

The inverted image should be used to determine the refraction only in cases of extreme myopia.

We find from his later work<sup>1</sup> that he has in no respect changed his opinion of the great value of the method. He states that the refraction at the macula and at the disc are almost always the same. It is only in great myopia, extreme hypermetropia, or aphakia, that the refraction differs at the two places; but the error so introduced can scarcely be greater than that which occurs in the use of glasses from the varying distance of the latter from the eye. He takes for his test-object the connective-tissue ring (scleral boundary) of the disc; the refraction for the macula lutea is the same.

What, then, has been the reason that a method which offers so many advantages appears to make no progress among the mass of ophthalmic surgeons? A few have, it is true, acquired great skill; the majority of specialists have not yet learnt facility in using, and accuracy of observation with, the erect image, both of which must necessarily precede it. Helmholtz's ophthalmoscope, with its glass plates and weak illumination, specially adapted for the purpose, was rapidly followed by a long series of instruments with reflectors of polished metal or silvered glass. The large field and brilliant illumination of the inverted image had no doubt much to do with the general preference. Two more influential hindrances to the frequent employment of the erect image were the necessary closeness to the patient's face and the frequent change of glasses, involving, as it did, some consideration of the refraction and much loss of time. The former cannot be avoided; the latter has to some extent been overcome by Loring's instrument, in which discs, containing a large number of glasses, are placed at the back of the mirror. Some modifications have been since brought forward, of which an account will be found in Loring's pamphlet, quoted at the beginning of this article. Wecker's instrument is very elegant, and has in our hands proved satisfactory.

These instruments will do much in popularising the use of the erect image; they all have, however, one fault—the observer is obliged to look obliquely through the glass, which consequently acts as a spherico-cylindrical lens, varying in power according to the amount of rotation. This is of no great moment where the lens is weak, but when the ametropia is high, or where great accuracy is desired, recourse must be had to the older instrument of Jäger, or the observer must hold the glass in its

<sup>1</sup> 'Vorlesungen über die optischen Fehler des Auges.' Wien, 1872. pp. 218—215.

proper position close to the eye observed, in front of, and not, as usually, behind the mirror.

This is not the place for an exposition of the principles of the method or for directions for practice; for both we refer the reader to the little work under review, in which the former are clearly explained, and some useful advice given on the latter.

The chapter on astigmatism deserves further notice. After describing the method of Knapp and Schweigger, the author proceeds:<sup>1</sup>

"From the uncertainty and want of delicacy attending this method of examination it is evident that, in order to make the ophthalmoscope of practical use in astigmatism, we must look for some more sensitive test to act either as a supplement to or a substitute for the one mentioned above.

"This we have in the vessels, and especially in the light streak on their centre of curvature. The streak begins to lose its brilliancy and its lateral borders their sharpness of definition the moment the vessel, particularly if of the smaller kind, becomes out of focus even to a very trifling degree. Low degrees of astigmatism, certainly as low as  $\frac{1}{16}$ , can be detected by this test, provided the accommodation in both the observed and observing eye is perfectly relaxed. On this account it is much easier to determine the defect if slight, when due to M than to H, and I do not think it is too much to say that even  $\frac{1}{32}$  can then under favorable conditions be pronounced upon.

"If we consider the optic disc as the centre of a circle, and all the vessels, large and small, radiating from it as so many straight lines, we have in the fundus of the eye itself a representation of Dr. Green's test for astigmatism, in which the principal branches of the central artery and veins represent the vertical lines, and the small vessels leaving the edge of the disc the horizontal and oblique. It may be said that the principal trunks of the central artery and vein do not always run exactly vertical. This is true, but such is their general tendency, and the fact that the vessels do not continue in their original vertical course is of itself an assistance to the diagnosis.

"The practical application of this is as follows:—If we look with the ophthalmoscope through the cornea of an astigmatic eye to the retina beyond, the effect is precisely the same as if we were looking through an astigmatic glass, and the vessels radiating from the optic nerve will then appear just as the radiating lines do in the common test when seen through a cylindric glass, *most distinct in the meridian of greatest ametropia*. This gives us at once the direction of one of the principal meridians, and we know that the direction of the

<sup>1</sup> Donders ('On the Anomalies of Refraction and Accommodation,' p. 489) says that ophthalmoscopic examination of the vessels running from the disc in the erect image gives the most certain indication of hypermetropic astigmatism. This method was employed in several of his cases; in one of Am. (p. 517), in one of H + Ah. (p. 526) & of Amb. (p. 529).

other must be at right angles to it. Having thus found out the direction of the principal meridians, we have then only to determine the refraction of each meridian separately, and the difference between the two will be the amount of astigmatism.

"If, for example, in a certain case the vertical vessels appear perfectly distinct, and are only rendered less so by glasses, one of the principal meridians of the eye must be emmetropic. If, however, the fine horizontal vessels are only made distinct by a concave  $\frac{1}{4}$ , then the second principal meridian must be myopic  $\frac{1}{4}$ , and inasmuch as the first was emmetropic, the amount of simple astigmatism present must be one twenty-fourth. . . . . The writer readily admits that this method is also, though by no means in the same degree, wanting in accuracy, and is not to be looked upon at all as a substitute for the trial by glasses, but is to be used in co-operation with it. When so employed, the ophthalmoscope often renders important service in revealing to us, at a single glance, as it were, the nature of the anomaly and the general direction of the principal meridians, when to have obtained them by glasses would have been an affair of hours." For example, "a young lad was examined by me, who, it was alleged by his parents, was nearly 'blind' in one eye. On testing the eyes the left was found to have a trifling degree of hypermetropia ( $\frac{1}{30}$ ) with vision one. In the right eye, however, vision was reduced to  $\frac{1}{50}$ ; that is, Snellen C could only be read in five feet. A few trials were made with glasses, with no material improvement in vision. In looking into the eye with the ophthalmoscope the nerve appeared distorted and drawn out vertically, while at the same time its outline was indistinct in all directions, as were also all the vessels. On using the accommodation, however, the vertical edge of the vessel became well defined, as did all the vessels, so long as they ran in a vertical direction, but as soon as they deviated from this they at once became indistinct, and in proportion to the amount of the deviation. This was very apparent at a certain place where one of the larger vessels divided, sending off a branch almost at right angles to the original direction of the vessel. The branch which continued in the vertical direction remained perfectly distinct, and the light streak in the centre of its walls clearly defined, while that running at right angles to it—that is, horizontally—was indistinct and evidently much out of focus, as were, in fact, all the vessels, large and small, running in this direction, and no amount of tension or relaxation of the accommodation made them clearly defined.

"It was manifest that astigmatism was present, and that the directions of the principal meridians were vertical and horizontal. It was evident, too, that as it required the action of the accommodation to make the vertical vessels distinct, that there must be hypermetropia in the horizontal meridian. In determining the degree it was found that the strongest glass through which a certain fine vertical vessel remained distinct at two inches distance was a convex  $\frac{1}{6}$ , the hypermetropia in the horizontal meridian was, therefore, equal to  $\frac{1}{16-2} = \frac{1}{14}$ .

"As the horizontal edge of the nerve and all the vessels running horizontally remained indistinct, even when the observer's accommodation was perfectly relaxed, it was evident that the rays which formed the horizontal boundary of the nerve and vessels must leave the eye as convergent, and as these rays are vertical rays, the eye must be myopic in the vertical meridian. It was found that the weakest glass under which the horizontal boundary of the nerve and vessels became sharply defined was  $-\frac{1}{10}$ ; the vertical meridian was, therefore, myopic equal to  $-\frac{1}{10+\frac{1}{2}} = -\frac{1}{12}$ .

"The case was, therefore, one of mixed astigmatism, in which the vertical meridian was myopic  $\frac{1}{12}$ , and the horizontal hypermetropic  $\frac{1}{12}$ , and the discrepancy between the two meridians was  $\frac{1}{12} + \frac{1}{12} = \frac{1}{6}$ . With a bicylindric glass  $-\frac{1}{12}$  and  $+\frac{1}{12}$  vision at once rose from  $\frac{1}{10}$  to  $\frac{8}{10}$ . It was, in fact, increased eight-fold. It was subsequently found, from a careful examination that  $-\frac{1}{12}$  c. and  $+\frac{1}{12}$  c. was preferred. With this glass vision became one half."

The next article on our list showing in a striking manner how the ophthalmoscopic estimation of the refraction throws light on a difficult subject, is one by Dr. Schnabel, in which he discusses the influence of spasm of the ciliary muscle in progressive myopia, the nature of the changes seen with the ophthalmoscope and found on dissection in posterior staphyloma, and the conditions on which the production of myopia ultimately depends.

Ophthalmic surgeons had become convinced before any exact knowledge of the subject had been gained—and the author quotes in support of this assertion Beger's work on short sight, published in 1845—that myopia (1) is usually acquired during growth, for it is rare in early childhood and frequent after puberty, and because educated persons can often assign the exact date when they ceased to see perfectly at a distance, and (2) that its genesis is materially influenced by the use made of the eyes, for its frequency varies in different classes of the same population and in different populations according to their cultivation.

These statements have been confirmed by Stellwag, Jäger, and Donders, and by the laborious statistics of Cohn, Erismann, Krüger, Hoffmann, supplemented as they were by the researches of Dobrowolsky, Hosch and Schiess. All agree that the manner and degree of use of the eyes influence myopia, but they differ as to the mode and amount of that influence. Stellwag, Donders, and Jäger hold that a necessary condition for the production of myopia is a congenital tendency of the sclera to expansion, whilst Dobrowolsky, Schiess, &c., reject such a view. The latter investigators say that acquired myopia is at first often only apparent, due to an increased refraction by the lens, which state is itself due to a tonic spasm of the



ciliary muscle, and that this pseudo-myopia passes afterwards into the permanent form with elongation of the eyeball. They consider the ectasy to be induced by inflammation and atrophy; sclerotico-choroiditis posterior. Dobrowolsky describes also a congenital form of myopia from malformation "transmitted from the parents to the child in the form of a really elongated optic axis" and not as a mere hereditary tendency.

In respect to spasm of the ciliary muscle, Dr. Schnabel has examined 210 eyes, of which 120 were myopic, 40 hypermetropic, and 50 emmetropic; the eyes chosen were those of young people, who for the most part were constantly occupied with small objects. Atropin was applied to 89 of these eyes (38 M, 17 H, 34 E.): the refraction remained after this application unchanged in 17 M and in 3 E. A tonic spasm was not found in any case.

The degree of H, measured by glasses, is often found to be different before and after the use of atropin, and in this sense we speak of it as partly manifest and partly latent. Measured by the ophthalmoscope, it is all manifest, providing the patient does not fix a particular object: as soon as he does this, he exerts his accommodation to a corresponding extent. The change of manifest into latent H can thus be directly observed with the ophthalmoscope by letting the patient alternately fix and not fix print placed at a distance of twenty feet. The contraction of the ciliary muscle agrees exactly in time and amount with its purpose, that of distinct vision.

The hypermetropia found after the use of atropin in E did not in any case exceed that due to suppression of the tone of the ciliary muscle.

The refraction determined by the ophthalmoscope was lessened in no case of M more than the amount due to the same cause, loss of tone. There were, however, five cases in which glasses showed a diminution, though with the ophthalmoscope the M was found to be unchanged. Similar cases have been previously described, as by Dr. Hock, where—3 was required for distance, whilst ophthalmoscopically the myopia was trifling, and by Dr. Just where a concave 5 was the glass for distance and the ophthalmoscope showed emmetropia. Hence it is clear, that the increased tension of the accommodation, obviously pathological as serving no useful purpose, is even in these cases limited to the very act of vision.

The amount of refraction due to physiological tonicities varies from  $\frac{1}{10}$ th to  $\frac{1}{15}$ th according to Donders. The author has found a variation in 17 out of 38 myopic eyes, in 3 out of 34 emmetropic eyes and in all the cases of hypermetropia (17 eyes) between the refraction before and after the use of atropin: no alteration takes place in M of high degree.

The diminution in the refraction when atropin is applied repeatedly for weeks or months is very possibly due to an alteration in the form of the lens. He justly remarks that congestion of the disc or retinal vessels can rarely be diagnosed by the ophthalmoscope, and that of the choroid never.

The author concludes the first part of his essay to the following effect: As to the reliability of the data furnished by an ophthalmoscopic examination of the refraction, various doubts, which have been started, require consideration. In the first place there may be an error from the observer's ignorance of the state of his own ciliary muscle. The reply is that he may easily relax his accommodation entirely, or he may use constantly one and the same fraction of it, provided he carefully selects the weakest concave or the strongest convex glass with which he can see distinctly the fundus. In determining the refraction by glasses, we take the weakest concave or the strongest convex glass to represent the refraction in question, although the patient states that he sees with *equal* distinctness through different concave or convex glasses. So likewise in estimating the refraction by means of the ophthalmoscope, we must try to avoid any error arising from unconscious exertion of our own accommodation, in that we do not rest satisfied with any glass by which we see distinctly, but change from glass to glass, weaker if concave, stronger if convex, so long as the distinctness of the image is not decidedly lessened. We should stop only at that glass, to which  $\pm 60$  cannot be added, without causing a decided indistinctness, which continues after it has been looked through for some time. When, however, the patient's eye is continually moving, so that only a hasty glance can be gained of any one part, especially if in addition the pupil is very small, if the media reflect strongly, or if there is astigmatism, the observer is liable to contract his own ciliary muscle in trying to follow the movements of the eye. In such cases the result should be distrusted, owing to the tendency to much over-estimate the refraction. There is no reason to fear an error from accommodation on the part of the patient. For the very reason that the method is free from this source of error, it is decidedly superior to a subjective examination, and on the same account it gives results which can never be obtained by the other method except after atropin. Latent H and very often pseudo-myopia disappear before the ophthalmoscope. Hence the instrument is quite indispensable in determining the part played by the accommodation in respect to the most distant point of distinct vision. It informs us as to the nature of the accommodative tension of the hypermetrope, lets us recognise the intermittent

spasm of the ciliary muscle, which we have already described, and it alone enables us to distinguish the latter from a tonic spasm. The fact that every emmetropic eye presents some degree of hypermetropia after the use of atropin, might easily lead to the erroneous opinion, that there is no real E, but that what is called by that name, is indeed a slight H masked by tension of the accommodation. The true refraction is, however, that of the eye in a state of rest, and not that present in paralysis of the accommodation: hence it is found not by the glass required for distinct distant vision after the use of atropin, but by the glass required for the ophthalmoscope before the atropin. If, then, E is found, this is the true refraction: if there is  $H \frac{1}{60}$ th, it can be diagnosed by means of the speculum before the use of atropin. Since the tone represents the physiological type of spasm of the accommodation, the same means of diagnosis by which we recognise it will decide as to its pathological increase. If the refraction, when the objective and subjective examination give the same results both before and after paralysis of the accommodation, is much less after than before the application of atropin, then, but then alone, is the diagnosis of a tonic spasm of the accommodation justified. Such being the decided advantages of the objective method of determining the refraction, we may assert that a full decision is not conceivable without its use. We must at once add, that it cannot take the place of the subjective method, and that it is but a help, though a very important one, in the selection of spectacle glasses.

We not unfrequently notice in slight anomalies of the refraction, that the patient chooses with certainty between two spherical glasses which differ very slightly in power,—thus between  $\frac{1}{30}$  and  $\frac{1}{40}$ , between  $\frac{1}{60}$  and  $\frac{1}{80}$ ,—and returns with a wonderful decision and constancy to the glass first selected, when the trial is repeated a number of times. The ophthalmoscopist, alternately using two glasses, so nearly the same as those mentioned, cannot perceive any difference in the distinctness of the image, for there are no objects in the fundus which would allow so accurate a judgment of the acuteness of the perception as the letters used in testing the vision. The most eligible plan seems to me, to seek the glass with which the striated retinal reflection can be best seen in the vicinity of the disc, if possible between it and the macula lutea. The difference in distinctness between two glasses varying  $\frac{1}{60}$  in power can usually be perceived with ease and certainty: the man of greatest experience will only exceptionally recognise differences, which are less than  $\frac{1}{120}$ , by a change in the clearness of the image. The objective method has also imperfections which cannot be overcome by the greatest skill. The assertion, which

has been often made, that the estimation of the refraction at the disc and at the macula lutea of the same eye may give a different result, has been completely confirmed in my practice; and I may add, that the results do not quite agree in many cases, even in respect to different parts of the same disc, the excavation of course being excluded. In forty-five cases, in which I determined with all possible care the refraction at the disc and at the macula lutea, both before and after the application of atropin, I found differences in five, which could not be neglected in practice, and of which the greatest was  $\frac{1}{5}$ . It is surprising to find that in all these eyes, of which 3 were myopic, 1 emmetropic, and 1 hypermetropic, the refraction was greater at the disc than at the macula. It is very possible that further researches will show that the percentage of such cases is less than would appear from the numbers mentioned, because I noted for some time every case that seemed paradoxical, but neglected the mass of those in which the results agreed. Very often the inner and the outer edge of the disc cannot be seen with equal distinctness through the same correcting lens. I often saw the inner edge of the disc in the emmetrope without a correcting glass with the utmost possible clearness, whilst the delicate vessels running over its outer edge and the connective-tissue ring on the same side seemed covered by a slight mist. The latter disappeared completely when I looked through a concave 60, whilst at the same time the inner edge became veiled by a yellowish mist. I once found in a myopic eye a difference of refraction of  $\frac{1}{4}$  between the inner and the outer edge of the disc.

In the second part of his essay, Dr. Schnabel discusses at great length the choroidal changes seen with the ophthalmoscope in myopia. He understands by posterior staphyloma the abnormal form of the eye<sup>1</sup> on which myopia depends, and by conus, the cone or crescent at the edge, usually the outer edge, of the disc. He states that Jäger and Schweigger have conclusively shown that the cone is not the result of an inflammatory process, of a sclerotico-choroiditis, and that von Graefe expressly adopted the same opinion. In respect to the influence of the accommodation, it is urged that the tension of the choroid is greatest at the macula lutea, and that the cone usually forms where the ciliary muscle can exert the least action, whilst it is rarely seen at the inner side of the disc, where the traction must be much greater.

Of the 210 eyes previously mentioned, 135 showed cones : of

<sup>1</sup> In the previous part of his paper Dr. Schnabel distinguishes simple elongation of the eye from staphyloma posticum; here he seems to confound them together.

these 97 or 73·33 were myopic, 18 or 33 per cent. were emmetropic, and 18 or 33 per cent. were hypermetropic. Dr. Schnabel cannot give the number of eyes examined, and consequently these numbers do not give the relative proportion of the cases. He is certain, however, that the number of *non-myopic* eyes with cones is far greater than that of *myopic* eyes without; that the appearance of a cone is exceptional in hypermetropia, where the accommodation is so much exerted, whilst it is rarely absent in myopia, where the accommodation is usually slight; that it is found in cases of hypermetropia without asthenopia, and that its size is in them not proportional to the hypermetropia or to the exertion of the accommodation, whilst it may not be found in other cases of hypermetropia attended for years by violent asthenopia; finally, that it may appear in myopic eyes in which the accommodation is scarcely employed, and be absent in cases where it is in constant use.

Two varieties of choroidal cone can be distinguished, one congenital and the other acquired. The congenital cone is usually continuous with the outer, sometimes with the lower edge of the disc: so far Dr. Schnabel has never seen it surround the whole disc. Its surface is occasionally of a dull yellow, usually, however, it is glistening, greenish; it is mostly quite without choroidal vessels or the grayish-black spots which are so common in the cones of myopic eyes from more or less atrophied stroma-pigment. Its size is rarely considerable; once only was it as large as the disc; ordinarily it is a crescent of the breadth of a large vein, equalling  $\frac{1}{8}$ — $\frac{1}{4}$  of the diameter of the disc. It occurs in eyes of every refraction, and Dr. Schnabel thinks with tolerably equal frequency; so far he has not noticed any predominance in myopic eyes.

In two cases of congenital cone, one in a hypermetrope and the other in an emmetrope, the author found that the punctum cæcum is enlarged by the extent of the cone. The cylinder of an ordinary oil-lamp was blackened, and then a spot, two to three lines diameter, was cleared. The light from this little spot was then reflected by a plain mirror on to the disc, and gradually moved over it and the cone. In this way the author proved that the *edge of the cone exactly coincides with the edge of the blind spot*. He comes to the conclusion that the cone is due in these cases to an imperfect development, which is analogous to the so-called coloboma of the choroid.

With respect to the *acquired cone*, which, according to the author, occurs exclusively in consequence of posterior staphyloma (see p. 56), it has been shown by dissection that the layer of pigmented epithelium is *entirely* absent, whilst the stroma is never more than partially wanting: the atrophy of the latter

never exceeds that degree in which a thin but separate membrane can be raised from the sclerotic. The atrophy of the stroma is not limited to the cone, but extends over the whole staphyloma: it is most marked at the apex of the ectasy, if we except the immediate vicinity of the disc; it ends without any definite edge, diminishing towards the equator, where indeed the sclera also is usually normal.

The author considers the changes of the stroma to be alone due to atrophy from distension, whilst he attributes the entire absence of the pigment layer to a recession of its edge from that of the disc, a displacement caused by the expansion of the globe. This explanation would give a reason why the edge of the cone next to the disc is marked in only exceptional cases spots of pigment, and never by a continuous dark line, whilst its outer edge is usually defined by a beautifully pigmented border.

The position of the cone depends on the direction in which the coats of the eyes yield, that is on the situation of the apex of the staphyloma. Jäger has found in the annular form that the apex of the distension was in the optic nerve, and that when the cone was placed at the inner and upper side of the disc, the bulging was also at the inner side.

Such being the state of the choroid, what is that of the retina? Von Graefe considered the enlargement of the blind spot by the extent of the cone to be an ordinary result in myopic eyes. With special reference to this subject, the author has examined fourteen myopic eyes in eight individuals, and found that in ten the blind spot included the whole surface of the cone exactly up to the border of pigment; whilst in four, owing to various difficulties, no certain conclusion could be drawn. It is interesting to find that the limit of the blind spot was accurately defined by the line of pigment even when no change could be seen in the choroidal stroma, where indeed the cone was characterised simply by a line of pigment which separated a semilunar space uncovered by epithelium from the rest of the fundus. So far these researches support the opinion that when the pigment layer is absent, the sensory layer of the retina is also wanting.

We learn then that, according to the author, the choroid is entirely absent in the congenital cone, whilst in the acquired form it is more or less atrophied. An ophthalmoscopic examination will often indicate by means of this difference the manner of origin. Both kinds occur not unfrequently in combination, so that the surface of the cone is divided into two parts, which may be distinguished very readily by their colour and by a sharply-marked curvilinear line. The inner portion, next to the disc, has no choroidal vessels, is white or greenish-white

and glistening: the outer part is blackish-gray, orange-yellow, or dirty-white, usually does not glisten, and allows choroidal vessels to be seen with more or less distinctness. The sharp, regular line separating the two part, proves that they are not both due to atrophy.

Other changes are not unfrequently met with in the immediate vicinity of the disc, which may be mistaken for cones. Where there is a doubt, it is well to remember that the chief characters of the true cone are the absence of the pigment line where the cone touches the disc and the increased size of the blind spot. Stripes of pigment which include normally coloured tissue are incorrectly called cones: so also are the dark or black cones figured by Jäger, which have no connection with staphyloma. Choroidal patches which are separated by only a line of pigment from the outer edge of the disc, have but an external resemblance to the true cone. We sometimes see in the otherwise healthy eye of the old, appearances similar to those described, but which are unconnected with staphyloma. The ring round the disc in glaucoma has rarely any great resemblance to the cone seen in posterior staphyloma.

Passing now to the third division of Dr. Schnabel's essay, we come to an examination of the various causes assigned for the scleral ectasy. In the first place we may assume the view to be probable that the cause is the same in all cases of posterior staphyloma. Dr. Schnabel concludes, for various reasons which he details at length, that the yielding of the sclera is not caused by an inflammatory process, a sclerotico-choroiditis. Even in acquired sclerectasy cases are met with where there is not the least reason to ascribe the bulging to inflammation. Increase of intraocular pressure is not the cause; the sclera resists far more than the disc, even in young people, for where the tension is increased, as in multiple anterior synechia, projecting corneal cicatrices, it causes an excavation of the disc, and not a staphyloma, whilst in posterior staphyloma there is no adequate cause for an increase of pressure, nor are there its usual symptoms.

A posterior staphyloma, great in degree, where there has been no effort at near-sight, no increased intra-ocular tension, no parenchymatous disease, must be ascribed to a pre-existent anomaly of texture. In other cases, by exclusion of causes, we come to the same conclusion, that the yielding of the sclera depends on some congenital peculiarity of structure. The frequent coexistence of myopia in near relations supports this view.

At the same time it has been proved by the statistics of Brismann and others that the development of myopia is con-

nected with the use of the eyes. We may easily suppose that if the tendency to staphyloma is very great, the process will begin, and even reach a high degree in early youth. On the other hand, if the tendency is slight, it may remain latent in those who scarcely ever exert their eyes, whilst a staphyloma forms in those who have much fine work to do during youth; yet a predisposition must be admitted, because but few become myopic of the many that are overworked. In the same way we may explain why large cones are relatively more common in people who never make much use of their accommodation than in the educated classes; in the former the staphyloma forms only when the tendency is very great, in the latter the continual use of the eyes makes the least tendency manifest, and large cones become relatively less common.

We may remark that Dr. Schnabel's paper has received the high approval of Jäger, who in his last work<sup>1</sup> states that he agrees in general, and even in most particulars, with the views there expressed. It alone, among all the works hitherto published on myopia, staphyloma posticum, &c., seems to him to follow the proper course.

Supposing the correctness of Dr. Schnabel's views, it would still remain for examination why a cone forms in some cases of acquired myopia and not in others. From some expressions in his essay, Dr. Schnabel would seem to hold that when the cone is absent the myopia is to be referred to simple elongation of the globe, and that when it is present there is a distinct staphylomatous projection. Thus, in writing on the distinction of the congenital from the acquired form of myopia, a distinction which he considers relatively unimportant, he says that elongation of the eye is in both forms sometimes accompanied by a staphyloma and sometimes not, and adds that about half the cases of the latter, or elongation without a staphyloma, occur in acquired myopia, the only means of distinction mentioned being "the choroidal changes characteristic of staphyloma posticum" (see p. 5).<sup>2</sup>

Should Dr. Thomson's results prove correct, the occurrence or non-occurrence of a cone depends essentially on the existence or non-existence of astigmatism. He believes that myopia, like hypermetropia, must be frequently congenital, and that it is rarely uncomplicated with astigmatism. He is satisfied that a crescent at the optic nerve is an evidence of astigmatism, and that by a study of its situation we may form a close approximation to the best and worst meridians of the cornea.

<sup>1</sup> 'Ergebnisse der Untersuchung mit dem Augenspiegel,' Wien, 1876, p. 42.

<sup>2</sup> The analysis of Dr. Schnabel's paper in the 'Ann. d'Oculist,' vol. 73, p. 268, contains several considerable errors.



In support of these assertions, he says :

"In the notes of 100 cases of crescents I find astigmatism combined with M in eighty and H in twenty, and in all of these after their correction under atropia, with all the most trustworthy methods of examination, the line of the crescent has been found to correspond with the astigmatic meridian of the cornea, when viewed by the erect method through the correcting glasses. In a portion of the myopic cases the axis of the cylinder has been found to coincide with the long axis of the crescent, whilst in the remainder it coincided with the long axis of the disc, and was at right angles to the direction of the crescent. In the highest defects the crescents were double at either side of the disc or circular. In all the cases of astigmatism with hypermetropia the axis of the cylinder coincided with the direction of the displaced choroid, and the crescents were as marked as in A with myopia; in two cases needing  $+ \frac{1}{2}$  cy. ax.  $90^\circ$  there were crescents extending downwards more than twice the diameter of the disc. In these marked instances, particularly in M, the entire disc seems dragged towards the crescent, so that it is no longer possible to see into the depths of the porus opticus, which appears overhung by the edge of the excavation; and this sliding appears to account for the change in the shape of the disc from a circle into a narrow ellipse."

On the other hand,

"When the M is simple, the patients enjoy high V with proper spherical glasses, suffer little or no asthenopia, and present eye-grounds free from crescents. I have before me the notes of such cases, from  $\frac{1}{2}$ ,  $\frac{1}{3}$ ,  $\frac{1}{4}$ ,  $\frac{1}{5}$ ,  $\frac{1}{6}$ ,  $\frac{1}{8}$ , and upwards in degree, with perfectly normal eye-grounds. I have notes also of cases of hypermetropia from  $\frac{1}{4}$  upwards, with no lesions, high, if not full V, and absence of all fatigue when properly corrected."

A single case of astigmatism in both eyes, in which the other meridian was emmetropic, is given; there were very large cones. In five persons the crescents were found in one eye alone, the fundus of the other being entirely normal, and in these the eye with a normal fundus was emmetropic, or even highly myopic, while the one with a crescent was invariably astigmatic.

In all cases the changes at the disc were proportional to the optical defect, a slight degree of astigmatism

"Being marked by a displacement of the excavation and the vessels, with a narrow crescent involving, perhaps, only the hexagonal cells, whilst high degrees were combined with large white crescents, straightened vessels, an elliptical shape of the disc, and a dragged condition of that part of the disc where it passed into the staphyloma."

He has, however, met with many cases of astigmatism in which there were no crescents, the vision being usually monocular owing to strabismus, amblyopia, or extreme differences in

the refraction, and suggests that binocular vision is to be considered a necessary condition. He is unable to say why in myopic astigmatism the axis of the cylinder coincides in one case with the choroidal displacement, and in another is at right angles to it.

The practical result of Dr. Thomson's researches is that he has found the correction of the optical defect by glasses sufficient to arrest progressive myopia in some patients, and to remove in many others an asthenopia which prevented any useful work.

"We are also provided with a trustworthy test for astigmatism in the observation of any signs of drag, even alight, that can be seen at the disc, and I constantly use these hints to determine, not only its presence, but its meridian. How important these studies are in a hygienic point of view may be perceived when we consider the hundreds of thousands of dollars spent in school furniture since attention has been called to increasing optical defects in school children, and when we consider that no change of chair or desk can be of any service to an asymmetric cornea in comparison with the cylindrical glass which will harmonise the refraction, and permit the muscles of accommodation and convergence to do their work with ease, as in emmetropia."

THOMAS WINDSOR.

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## VI.—The Geography of Disease.<sup>1</sup>

WHATEVER difference of opinion may obtain regarding the intrinsic value of the results arrived at by Mr. Haviland, in respect of the geographical distribution of the several diseases he has taken in hand to investigate, there can be no question as to the merit attaching to him for the laborious work he has undertaken, and for the enterprise shown in placing his views and the basis for them before the profession, in a publication like the present, which, whilst commendable for execution, is, at the same time, costly in production, and can scarcely be anticipated to prove a pecuniary success.

Happily for Mr. Haviland, and creditably to the Government, a grant was accorded him for his 'basis map,' exhibiting the registration divisions, counties, and districts; a document which has obtained official recognition, and been employed in collecting the facts included in the last census. It is likewise worth

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<sup>1</sup> *The Geographical Distribution of Heart Disease and Dropsy, Cancer in Females and Phthisis in England and Wales.* Illustrated by six small and three large coloured maps. By ALFRED HAVILAND, M.R.C.S.E., &c. London, 1876, pp. 116.

noting that the engraved copperplate of the map is available to Medical Officers of Health and to Sanitary Authorities who desire to obtain an outline map of their respective areas of administration.

The author justly claims for the geographical distribution of disease, that it is "essential to a practical knowledge of preventive medicine; for it involves the study of the physical, geological, meteorological, and other natural and social characters of the country or district, the diseases of which are the subject of inquiry." And we will add, that if such a study be made by sanitary medical officers in their several districts, and the results be carefully recorded after a common plan, we shall secure satisfactory data for illustrating the geography of disease and the climatology of our country. In fact, instead of a rough, approximative representation, which at most can be reached by analysis of the official returns of mortality, we shall acquire something much more approaching accuracy, and more definite in its instruction. However, in the absence of these fuller and more precise materials, Mr. Haviland has made the best use possible of the returns of deaths from the several diseases investigated, as officially named and represented in the reports of the Registrar-General.

It would be no difficult task to illustrate, from the registers of any locality chosen, the many defects and errors that attach to the certificates of deaths from which the figures and conclusions found in the returns of the chief office have to be concocted and reduced to order within the compass of the authorized classification of causes of death. In a large proportion of deaths registered there is a concurrence of several lesions or diseases; and, even where ignorance does not mar the diagnosis, it is often a toss up which shall be returned as the cause of death; and not unfrequently it becomes a question whether, ignoring or overlooking the actual cause, the death shall not be placed to the account of one of those very wide and elastic categories recognised in the official nomenclature,—so admirably adapted to conceal ignorance,—atrophy, debility, convulsions, thrush, or privation. And even when a particular organ is settled upon as chiefly concerned in inducing death, we frequently gather but a very imperfect conception of the actual morbid condition, owing to the fatal event being registered under such general headings as, diseases of the brain, diseases of the lungs, dropsy, phthisis and asthma, &c. For instance, under the last-named designation, experience has shown that deaths may be registered which arise from a variety of thoracic lesions, as well of the heart as of the lungs. And so in like manner with regard to entries of deaths from dropsy,

they may refer either to cardiac or renal lesions, or to the two combined, or possibly to ascites and hepatic disease. Even the word phthisis opens too wide a scope for exact registration, for it is customary to include several varieties of pulmonary lesions under the designation of consumption or phthisis; and in the records of the deaths of children, especially those from tubercular disease of the lungs, the returned cause may figure in the category of scrofula, of tabes mesenterica, of atrophy or debility, and probably also of other conditions representing symptoms or complications.

It is, however, needless to dilate on the errors inseparable from the registration of disease as at present conducted, and under the nomenclature patronised. It is Mr. Haviland's misfortune that he has no other and better basis for his investigations placed within his reach. And this much may be generally allowed, that the like errors are diffused at about an equal rate in all parts of the kingdom, and consequently affect results in almost an equal degree at all places and from year to year. Moreover, we would remark, that calculations extended over a longer period than ten years would add to the value of the conclusions arrived at; and that those made for a decenniad subsequent to the one employed by the author, may fairly be expected to possess increased accuracy, by reason of more careful and correct registration, and of improvements in medical practice.

It is not possible within the limits of this notice to present a complete analysis of Mr. Haviland's inquiries and conclusions. The actual matter of his treatise itself represents the stages of a progressive analysis of the materials afforded him, and of progressive deductions obtained therefrom; consequently, to fully represent the work he has accomplished we had need transfer most of the contents of his pages to our own, a proceeding neither desirable nor necessary when our readers can obtain the volume for themselves.

Nevertheless his conclusions are too important to be allowed to pass without remark, and it would be no compliment to him if we did so. His industry deserves a better fate than to be treated with indifference. We may at the outset observe that the deaths assigned to heart disease and dropsy have, by their number, one necessary element for the prosecution of investigations such as those under notice. For these inquiries are necessarily mixed up with statistics, and where such a question as that of the proportion of deaths to population and their distribution in space is concerned we cannot deal with security with small numbers.

During the ten years from 1851 to 1860, 236,973 deaths

were attributed to dropsy and heart disease; and of these 109,527 were males and 127,446 females. Allowing for the relative proportion of the two sexes in the population, the ratio of females to males was 13·1 to 11·8, to every 10,000 persons living. But age modifies the apparent sexual influence; for during the first ten years of life more males than females succumb to the malady; but after that decade and onward the female mortality rules higher, until between 75 and 85, when the deaths of males again preponderate. "Moreover, after 25 the relative mortality in both sexes nearly doubles itself in each succeeding decade, up to that commencing at 75, when only an increase of one-fifth in the mortality takes place," terminating in the next 15 years in a positive decrease.

Mr. Haviland's next business is to examine the mortality in connection with its diffusion in the eleven 'registration districts,' and the inferences arrived at requiring notice are:—that the mortality from the diseases in question stands in no direct, but rather in an inverse ratio to density of population, and that the causes regulating the general mortality of England are not identical with those which influence the geographical distribution of heart disease. Advancing a step farther in his analysis of the distribution, he arrives at the conclusion, that the two Midland districts, and 3 of the nine coastal have a high rate of deaths; whilst the six remaining coastal enjoy a low mortality from heart disease. The unfavourable coastal districts are distinguished from the others by "a precipitous and rock bound coast having few inlets, which, when they do exist, are, like the courses of their river and valley system, at right angles to the prevailing winds and the current of the tidal wave. These characters are predominant on the coast of the northern counties division as well as on the two southern," but have their opposite on the eastern and western coasts of the island.

So likewise when an examination of the several counties is made, there is a like coincidence between the mortality of heart disease and the geographical features, and one of a more definite character; those counties most exposed to the prevailing winds and sea-air exhibiting the least, and those most protected by physical surroundings the greatest ratio of deaths. The same law obtains when the inquiry is extended to smaller areas, viz., the 133 registration divisions; and even in the case of towns, as for example, London, where exposure to currents of air is regulated by the direction and freedom of streets, the ratio of deaths from cardiac disease stands in direct relation with their free flushing by the prevailing winds. With most praiseworthy industry and immense pains, Mr. Haviland has prepared a map

of England on a large scale (1 inch to 12 miles) in which each registration division is marked out and coloured, in harmony with a fixed plan, in different shades of red and blue according to the death rate of heart disease. Examination of this map shows 17 high mortality areas in the south and south-western districts and one in the West Riding, the Otley and Wharfedale registration division. These all agree in having an agricultural population much above the average, and in being sheltered spots to the leeward of some protecting range of hills.

We have now sketched the principal conclusions Mr. Haviland would draw from the statistical returns of the mortality of cardiac disease analysed by him in relation to its distribution, and which are admirably conveyed to the eye pictorially by his excellent map. We have followed the steps of his demonstration, which resembled an inductive process, but we regret we cannot assent to the outcome as proven. A general examination of the map at once suggests numerous doubts; and when a closer scrutiny is made, with local knowledge of physical features, of prevailing diseases, and of particular causes of disease, those doubts multiply, and reduce credence in the conclusions to small measure. We are, nevertheless, not indisposed to recognise the importance of free exposure, especially to sea-air, in reducing liability to cardiac disorders; and also the converse, that valleys shut in, with stagnant air and a retentive soil productive of humidity, may predispose to rheumatism and its frequent com-comitant, heart disease, and the more so when the population is ill-clad, ill-fed, and exposed to weather.

In his concluding summary the author adopts the "opinion that the great majority of heart disease cases in our country have their origin in rheumatism, . . . but that it does not require a regular attack of rheumatic fever in order to set up disease in the heart;" for his experience leads to the belief that rheumatism is endemic in many places, and that it attacks insidiously children in early life, when, from being often unnoticed, it is allowed to produce serious cardiac mischief, and those chronic sufferings in later years in the joints so commonly seen among agricultural labourers. He further entertains the belief that his researches point to some *materies morbi* (i.e. of rheumatism), resident in certain localities, perhaps in all, the difference and extent of its operation being determined by the amount of air-flushing in different places, which will act beneficially not only physically, but also chemically, and probably so by the presence of ozone. Lastly, he considers that low wages and low living, with exposure to early morning labour, when the air is most chill and the exhalations of the night most noxious, are accessory causes of rheumatism especially prevailing in agricultural com-

munities. These opinions cannot claim general assent. They are open to many strictures.

"Heart disease," as Mr. Haviland says, "has almost every variety of cause imaginable," and, with this well-attested truth before us, we doubt "that the great majority of heart disease cases in our country have their origin in rheumatism." The frequent concurrence of the two is undeniable; but in strict language, the cardiac lesion is not the consequence of rheumatism, but the two morbid states are results of the same cause. Pathologists call rheumatism a "diathetic" malady, and consider it hereditary; if, consequently, they are right, there are conditions favouring its occurrence beyond what is explicable by the geographical position of abode. Again, the distribution of rheumatism in space is extremely wide, and not explicable by the physical features of the countries in which it prevails, or by the extent of free exposure to sea-breezes. There is more rheumatism among our troops in the islands and stations of the Mediterranean, and in the West Indies than there is in the bleak, and cold, and damp regions of Nova Scotia, or in Canada. Indeed, there is less of the disease in countries bordering on the polar regions than in temperate and hot climates. The most reliable interpretation of its occurrence is, not mere exposure to cold, but exposure involving great variation of temperature, particularly in the presence of cold moist air, with insufficient clothing, and when the vigour of the system has been reduced by debilitating agencies. It is a very common malady among the Arabs, who make very little difference in their clothing at any period, and expose themselves insufficiently sheltered to the cold and heavy dews of night in the desert. Its maximum of prevalence in England is in November and December, which are usually milder months than January and February.

As to rheumatism being the cause of most cases of heart-disease and dropsy, so registered in the official returns, it is very doubtful. Those returns furnish no material for or against the proposition, but experience reminds us of the many defects that lessen their value as records of the true causes of death. Mr. Haviland takes heart-diseases and dropsy together, and consequently exposes himself to more errors; for deaths attributed to dropsy imply a want of differentiation by those who so return them. It is but an assumption, and a loose one, to suppose that the cases of dropsy in the 'Registrar-General's Reports' are all cases of anasarca from cardiac disease. Again, the acute diseases of the heart acknowledge many other causes than rheumatism. According to Dr. Fuller, the heart complication occurs in rather less than one in six cases of acute rheumatism. As a common cause of chronic endocarditis the abuse of alco-

holic drinks is universally acknowledged. But besides these inflammatory affections, every practitioner meets with a host of cases of heart degeneration among people advancing in years, and among sufferers with Bright's disease, gout, and syphilis, and, in our opinion, cases referable to these conditions are more numerous (certainly so in the later stages of life) than those traceable to rheumatism. And this leads us to remark that the somewhat preponderating ratio, noted by Mr. Haviland, of heart disease among females in comparison with males, may be partly attributed to the known greater and earlier tendency of the former to degenerative change, and partly, likewise, to the fact of their greater proclivity to suffer from cardiac complication when attacked by rheumatic fever.

The experience of Mr. Haviland, as expressed in the following paragraph, does not coincide with that of the most distinguished writers on heart disease and rheumatism. He says—

"The rheumatic heart disease which kills in such large proportions, is of insidious growth, often unsuspected in youth, and frequently allowed to remain unheeded until it has taken too firm a root to be removed. I believe it does not require a regular attack of rheumatic fever in order to set up disease in the heart, my experience extending now over nearly a quarter of a century in hospital and private practice, leads me to believe that in certain localities rheumatic disease begins to show itself very early in life, and that from neglect of the first symptoms much mischief ensues."

Now, there is a *primâ facie* weakness in the assumption here made; that because heart-disease is discovered in later years it has existed since childhood, but been overlooked and unheeded. Some evidence of it need be adduced; supposition is not evidence. Experience certainly teaches us all that valvular disease, hypertrophy, and other lesions of the heart, may and do occur in persons who have never had rheumatism; but this fact only shows that such lesions own many causes other than rheumatism, and it does not at all help Mr. Haviland's argument, while it tells against his general hypothesis of the maximum influence of rheumatism in developing cardiac disease, just in proportion to the frequency established of the operation of those other causes.

Further, with regard to his belief of the common occurrence of insidious rheumatism in early life, nothing for or against it, as an unknown or unrecognised condition, can be directly established, but if we look to the statistics of acute rheumatism—a sufficiently recognisable condition—we find it distinctly made out that it is rare in childhood and youth, and consequently we are less prepared to admit the reality of Mr. Haviland's supposition with regard to the frequency of an insidious form. In



Chomel's experience only two instances of acute rheumatism occurred among those under fifteen years of age among seventy-three observed of all ages. To controvert the statistics of the French pathologist, Mr. Haviland may point to his table, showing the statistics of deaths at various ages from heart disease and dropsy, as indicating a mortality from those causes among children under five of 3·8 to 10,000 living, of one of 2·2 between five and ten, of 2·5 between ten and fifteen.

If we read the table aright, we gather from these statistics that in a population of 10,000 under fifteen the mean mortality from heart-disease *and dropsy* equals 2·8. This, indeed, represents no extraordinary prevalence of these maladies in childhood and early youth, and when we remember the terrible mortality of infancy and early childhood, the ratio in question loses its significance.

Again, it is possible that insidious rheumatism may exist and gradually develope heart-lesions after the same fashion as gout does, but we have it is an admitted rule that the cardiac complication of rheumatic fever presents a direct relation to the severity of the attack and the degree of fever.

The reputed high mortality from cardiac disease and dropsy among old people and in agricultural populations admits of some explanation apart from geographical position and the extent of air-flushing. Agricultural labourers are exposed to atmospheric conditions always recognised as associated with rheumatism. Their early morning exposure is rightly noted by Mr. Haviland, whose statement has been previously quoted. But besides this we need notice their generally ill-clad condition, the same clothes being worn in all weathers; their partially stripped state in some labour, and the nature of portions of their work, such as ditching; all of these conditions favourable to producing rheumatic attacks—as well rheumatic fever as muscular rheumatism. Moreover, a moderate acquaintance with practice, particularly in rural districts, will make us aware of the number of worn-out labourers, whose chief complaint, perhaps, is rheumatism or rheumatic pain in more or fewer parts of their body, but who are mostly also sufferers with anasarca, with lung-congestion and weak, if not organically diseased, hearts, together with old bronchitis, failing kidneys, and other indications of lapsing life. Suchlike cases will greatly multiply the lists of deaths returned as from dropsy and heart disease, although these are but concomitants or sequelæ of other morbid states and symptoms of a general breaking-up. Cases of this kind are common in every part of the kingdom, whatever its geographical position, and when they are numbered up in a sparse population, and their ratio to the whole

local population, or to its mortality, is worked out it is not surprising if heart disease and dropsy appear to preponderate, and, for the reasons already given, the more so in agricultural communities.

Air-flushing or, as he elsewhere expresses himself, "the dynamical element of wind," has doubtless great merits, but we cannot realise the vast difference in its energy or operation, as represented by Mr. Haviland, on the east and west coasts as compared with the south, even when we take into account, as desired by him, the number and course of the rivers on those several coasts. For, although there be not on the south coast such large rivers as the Thames, Humber, and Tyne, existing on the east coast, there is, proportionately to the coast-line, a larger number, and, besides rivers, very numerous valleys debouching seawards, which must admit free currents of sea-air to the interior of the country. With what force the south-westerly sea-breezes drive landward it is only necessary to ask our sailors in the Channel, or to visit the exposed south coast, to notice the trees in the 'combes' of Dorsetshire, unable to raise their heads above a certain level in the sheltered hollow in which they have managed to take root. And it is a mere assumption to represent the south-coast as bounded by high cliffs in contrast with a low elevation of the east and west coasts generally. It is, as it were, forcing nature to sustain a hypothesis.

Lastly, in estimating the author's opinions relative to the importance of winds and of the ozone they are charged with, it is well to remember that the prevailing winds of this country, for the greatest part of the year, are the west and south-west, of which necessarily the southern coast of this country gets a greater share; in other words, a more direct flushing with sea-air than the east coast; although, speaking generally, this latter is declared to be remarkable for a smaller ratio of deaths from heart-disease and dropsy than the northern border of the kingdom. And, with regard to the author's notion as to ozone, if experimenters are to be believed, this material is much more abundant in westerly and south-westerly winds than in the east wind. Consequently, the coasts upon which those former winds directly blow should have the advantage of a higher dose of ozone, and the eastern coast be deficient of it; and yet, says Mr. Haviland (who himself calls the east wind an "effete air,") the east coast, flushed by a poorly ozonised air, has less rheumatism and less heart disease and dropsy, and yet, withal, he argues that ozone is a material cause in the prevention of heart disease.

We will enter no further into discussion on this essay on the

geography of heart disease and dropsy. We think we have said enough to raise considerable doubt respecting its conclusions; at the same time, we do not profess to have disproved that there are relations subsisting between geographical position and the prevalence of those maladies. In fine, we consider we have still to await the determination what those conditions precisely are.

On the two remaining essays we cannot expend the like time and space. Many of the same objections obtain with respect to the mode of inquiry and the results deduced, which have been already urged in the review of the first essay.

Owing to the unequal distribution of cancer between the two sexes, and its great preponderance among women (it being slightly more than double that amongst men), Mr. Haviland restricts his investigation of its geographical diffusion to female lives; and he estimates that one forty-eighth of the total mortality of women is referable to it.

He cannot discover "any relation of the mortality from cancer to the density of population, nor yet to the general mortality from *all causes*," but he believes a direct one to exist where there are rivers flowing through alluvial and sheltered valleys and plains which they are prone to flood. On the contrary he holds that, where rivers flow through narrow channels, bordered by hard rocks, and where the neighbouring country is freely exposed to the drying influence of the wind, there cancer mortality is at its lowest. The valley of the Thames with its tertiary soil and frequent inundations is a "typical cancer field."

Such is the general outcome of the examination as conducted by the author. We regret to say, that we attribute very small value to it as a contribution to the geography of cancer. If we look to the large map exhibiting the registration districts, coloured according to the ratio of cancer mortality in them severally, the impression derived is, that no such law as is enunciated can be legitimately deduced. Examples in its favour are matched by about as many telling against it; with the knowledge we possess of particular localities we fail to follow the author in his statements and deductions regarding them. Even in describing the wider geographical areas we cannot acquiesce in his remarks. Thus it is new to us to learn that Denbighshire contains the protected basin of the river Dee, for our maps show that county to be separated from the valley of the Dee by the breadth of Flintshire, and that it is freely exposed to the sea, having only one intersecting river, the Clwyd. Again, to describe Shropshire as a sheltered county, of which the valley of the Severn is the representative portion, is to distort facts; for, if the Severn in its upper course after leaving Wales

does traverse the county in a south-easterly direction, the area of its alluvial valley constitutes but a very small portion of the county, which in fact, is remarkable by its extensive hill country, as at Church Stretton, Ludlow, Wellington, and elsewhere.

If further, we turn to registration districts in search of examples adverse to the hypotheses advanced, we find them on all hands. The district of Hoo, placed between the two rivers, the Thames and the Medway, notwithstanding the very wide extent of its marshy and often inundated soil, ranks with districts the most exempt from cancer; whilst the peninsular districts of Thanet, freely exposed to the sea, consisting of highland, chalky hills, with a few mere strips of marsh, and with no rivers, occupies the second grade of mortality above the average, separated by three grades from that expressive of the prevalence of cancer in Hoo. For another instance, we may quote the inland district of Ticehurst, in the Weald of Sussex, having one small stream coursing through it, and no marshes, and sufficiently upland to receive a full share of sea-breezes blowing across the adjoining sea-board district of Rye; yet this ranks among the worst cancer localities. On the contrary, the district of Rye, through which the Rother flows in increased volume, and through a wide marshy, sheltered valley until it reaches the extensive Rye marshes, is presented to us as a district second only to those most exempt from cancer. It will not do to urge that the situation on the coast line explains its greater freedom from the malady, because the map shows us that such a position is no security against its prevalence; for almost the whole coast line of Kent, and much of that of Sussex is tinted as very cancerous. If we take Midland districts, we notice Stafford to be marked by the highest degree of cancer mortality; but on one side of it we find Stone, and on the other Penkridge, though similar in riparial characters and exposure to inundations, so coloured as to intimate a degree of mortality one and two degrees below the average.

It is useless to multiply examples adverse to Mr. Haviland's conclusions; they could be adduced from every quarter. We know how profitless is the attempt to convince a man against his will.

To conclude with a few general remarks on this cancer question. We consider cancer to have been unfortunately chosen as the subject-matter for investigation. The mortality from it compared with that from all diseases bears an insignificant proportion; and this being the case there is need, if reliable results are to be attained, to extend the analysis of its statistics over a much longer period than ten years. In small populations,

and especially in sparsely peopled districts, the occurrence of half a dozen or a dozen more deaths from cancer in one decenniad than in another would materially affect the calculated ratio to the total deaths, or to the whole number of individuals living. A cancerous family in a village might seriously stain its character; for, granting the malady to have an hereditary nature, the ill reputation that would befall it would not be a fault of geographical position, but of the presence of the disease-bearing family.

Wales appears wonderfully free from cancer, and we might speculate on the influence of race did we not note also that Lancashire is on a par with the Principality, although not the peculiar abode of Welshmen, and that throughout the map no distinct relation to racial peculiarities is cognisable. Another cause affecting its distribution suggests itself in the existence of hospitals; but the influence of these institutions is, at the most, extremely small. The cancer hospitals of London may possibly add to the calculated ratio of deaths in the districts where they are situated; but, as to hospitals and infirmaries generally, it may be remarked that, if they attract cancer patients, it is for the purpose of operation, and that few such patients die within their wards. Lastly, one fact admitted by the author does certainly not help him; that is, the equal prevalence of the disease among *males* in all localities alike. If geographical peculiarities exercised the power Mr. Haviland imagines over the production of cancer, they ought to exhibit their disease-working energy in the case as well of males as females. To say, as he does, that their failure to do so is a consequence of the out-door habits of men and the in-door habits of women is but begging the question or burking it. The in-door habits of women rather assimilate their conditions of life in all regions of the kingdom.

In fine, we fail to comprehend any better the etiology and pathology of cancer for all the pains taken by Mr. Haviland to illustrate its prevalence geographically. Its greatly more frequent occurrence in females and in certain organs and tissues, and its proneness to become hereditary, are facts suggestive of pathological action in which geographical features play no appreciable part. And this conclusion, too, we should arrive at from a close inspection of Mr. Haviland's map delineated to prove the contrary.

There now remain for discussion Mr. Hairland's conclusions relative to "the geographical distribution of phthisis in females." He tells us that he selects females for his research, because the deaths from the disease among them are sufficiently numerous to form a basis for investigation, and because he wishes to

"compare the geography of this cause of death with that of cancer among the same sex."

Phthisis is set down as causing, during the decade 1851—1860, 508,923 deaths, rather more than half of which, viz. 269,918 occurred among females. On colouring his outline map, according to his usual plan, to illustrate the relative mortality of females in the several registration divisions of the kingdom, and on comparing it with a similarly constructed map for heart disease, the author is at once struck with the fact that the one map is the reverse of the other in the distribution of its tints. The blue regions of the former occupy the red of the latter, and *vice versa*. And it appears to him that the regions unhappily tinted blue, by reason of their excessive mortality from phthisis, are those exposed to the "dynamic element of the atmosphere," i.e. air in motion, as wind. "They are the least protected and least sheltered of all the eleven divisions. The inference consequently is, that "free exposure to the force-element of wind is coincident with a high mortality from phthisis;" an inference, it will be remarked, the converse of that arrived at with regard to heart disease and dropsy. "London and the West Midland Division have the lowest female mortality" (24.9 to every 10,000 living), whilst the North-Western Division (Lancashire and Cheshire) has the highest. This latter "fact is coincident socially with the engagement of the population in cotton and other factories, and clinatically with exposure to the direct and powerful influence of the north-westerly winds. The map of heart disease and cancer show an exceedingly low mortality from these causes in this division;" and also in Wales, where likewise phthisis prevails above the average. Indeed, Wales ranks next to the north-western district in its mortality from this scourge. The insular county of Anglesey has the sad pre-eminence of the highest ratio observed. Having formulated his conclusions with regard to the larger registration divisions, Mr. Haviland proceeds to fit them to the geographical peculiarities of counties, and in this process betrays the hand of an ingenious artificer rather than of an unbiassed, calm philosopher.

According to his recapitulation :

"1. The north-western counties, the Welsh, and the midland counties, which have the most elevated ridges of hard, unproductive carboniferous limestone, or silurian, or other older formations, have the highest mortality. This is the reverse of what obtains in the geographical distribution of cancer and heart disease. 2. The south-eastern counties, which have a high mortality, are characterised by elevated chalk-ranges and villages in which the oolitic, the cretaceous, and wealden clays predominate. 3. The eastern counties, having a high mortality, are exposed in aspect to the easterly wind ;

and the lower lands are characterised by clays of the Eocene period, especially the London clay. 4. We therefore see that high, dry, chalky sites, exposed to the free access of the east winds, are accompanied by a high death-rate from phthisis; and that the same death-rate obtains in the cold, damp, clayey villages which these chalk ranges shelter."

On the other hand, the low mortality counties are sheltered areas, and "to the west of 1° W. longitude," in geological formation consist of either old or new sandstone. Lincolnshire forms an exception to the rule of exposed sites and high mortality from phthisis, somewhat, he thinks, explicable by the presence of ague (as antagonistic to consumption?). He urges likewise that, estimating the importance of this exception, attention should be directed to the fact "that the sites have been reclaimed from the sea." Lastly, "the protected sites of the North Riding and Northumberland have a low mortality from consumption, and the reverse from heart disease."

This seeming process of ratiocination crumbles to pieces when subjected to close examination, alike when the geographical and geological features of counties, and, still more, when those of registration districts are duly taken into account.

We have just seen how the author has to resort to his ingenuity to explain why Lincolnshire is an exception to the assumed rule, and to adopt suppositions which his map, on examination will not bear out. Again, the districts of Guisborough, Whitby, and Scarborough, and likewise those of North Northumberland—all which assuredly get the full "dynamic power of the atmosphere," and present "elevated ridges of hard unproductive limestone, or silurian, or other older formations," are yet withal regions of the lowest mortality from phthisis, and glaring exceptions to the law laid down. To such exceptions may also be added the exposed districts of East Kent, with their chalk hills, and the breezy rocky promontory of Caernarvonshire; and in contrast to these should be phthisical districts, we might cite the sheltered districts of Christchurch and Westbourne, Hants, and a group of registration areas in Huntingdonshire, Cambridge, and Suffolk, where the deep blue tint intimates the highest mortality from phthisis. Like these, too, the most deeply tinted Isle of Anglesey is suggestive that wind is not the prime factor, for in the example of this island the same ratio of mortality is indicated on its easterly protected side, along the Menai Straits, as well as on the opposite side of those straits in Caernarvonshire, as on the bleakest headlands.

Nor would any one unbiassed by theory discover, from the author's large map, evidence of the influence he attributes to the

wind. Taking the north-western division, Lancashire and Cheshire, with its registration districts, which surpasses all other divisions of the kingdom in the mortality from phthisis, it cannot be predicated rightly of it, that it is pre-eminently exposed to the force of the wind ; it is less so, indeed, than the East Coast of England, from Berwick-upon-Tweed to the Mouth of the Humber ; and less so than Cornwall and North Devon, all which regions have nevertheless a mortality from the malady two or three degrees below the average. In fact, on looking to the registration districts in Lancashire and Cheshire only, the notion is seen not to be supported ; for two of the most exposed districts of the former county, Fylde, with Blackpool as its chief town, and Ormskirk, with Southport, appear as having a mortality beneath the average, whilst the much more sheltered south and east sides of Morecambe Bay are remarkable for an excess of deaths from phthisis. So, again, in Cheshire, the most exposed portion of the county, the promontory of Wirral, between the estuaries of the Mersey and Dee, figures as having a mortality of the second degree under the average, whilst the interior sheltered flat plain of Cheshire, and particularly the districts of Congleton and Macclesfield, present tints indicative of each of the three degrees of mortality, from the lowest to the highest above the average.

It might be supposed that the quality of the wind when added to its dynamical power would be found to be a concurrent morbid agent, and that an east wind would be worse than a westerly one, but Mr. Haviland's map lends no support to the supposition ; for, so far as it can teach, it represents the exposed east coast to be generally as free from phthisis as the west coast. Nor does the element of moisture of atmosphere appear from the same datum to exercise an appreciable influence upon the mortality from this disease. Its preponderance in Lancashire on the moist west is matched by Suffolk on the dry east side of the island. And on comparing together regions on the same side we find them to differ widely in the ratio of deaths though similar in hygrometric conditions.

The hypothesis appealed to by the author to escape the difficulty presented to his conclusions respecting the morbid agency of wind in the instance of Lincolnshire, that where ague prevails phthisis declines in mortality, gets no countenance from his map, which reveals a high phthisical mortality in the fens of Huntingdonshire and Cambridgeshire, where ague is even more common than in Lincolnshire, and where, too, most of the soil has been reclaimed from swamps.

The contrasts the author seeks to draw between the geogra-



phical areas of the several diseases investigated, are not borne out on examination. He insists on the opposite geographical conditions subsisting between phthisis and cancer, and between the former and rheumatism. If free air-flushing, or the dynamic power of wind, be antagonistic to rheumatism and heart-disease, one might expect, as is the case, that Guisborough and Stokeley, in the North Riding, the promontory of Partington in the East Riding, and the far out-stretching exposed district of Penzance, and many other localities, enjoying such flushing, should exhibit a mortality under the average from those diseases; but the maps show also the same fact with regard to both cancer and phthisis in those localities, although phthisis should, by reason of such exposure, present a high rate of mortality, and contrast in this point also with cancer. So, again, the author would argue from his colours that both Lewes and Rye, in Sussex, are rheumatic regions because of the want of wind; but the like data should at the same time instruct him that they are very phthisical localities because they have too much wind. So with Cardiganshire, it appears both a rheumatic and a phthisical district, and involves the same conflicting reasoning.

The district of Machynlleth, in Wales, which being cancerous should not be consumptive, happens to be both; and a further similar example, among many adducible, is furnished by Shrewsbury. Lastly, the exposed promontory of Caernarvon has little heart disease, little cancer, and still less phthisis in its bills of mortality.

Mr. Haviland urges, as one useful lesson to be learnt from his researches, that they should teach individuals predisposed to certain diseases what localities to choose and what to avoid. Apparently a place to avoid above all others are the Scilly islands, which by their deep blue colouring warn us of the high mortality there incident from each and all the diseases investigated,—heart disease, rheumatism and dropsy, cancer, and phthisis. How this can be if the propositions we have examined are true, we must leave their advocate to explain.

In reading off the lessons his maps seem to convey, and in unfolding his hypothesis, the author is evidently carried away by foregone opinions, and to establish these falls into many ambiguous statements, whilst he keeps from view others that would be adverse to them.

This must have been seen from previous parts of this review. As a further specimen we may instance the statement hazarded respecting Sussex and Hants to account for the high rate of mortality from phthisis those counties exhibit,—wherein he describes as “more or less exposed to the direct influence of the

east wind," a circumstance their geographical position does assuredly not bear out. So again, finding Bedfordshire to have an excess of deaths from the same cause, he refers the circumstance to the chalk downs which run through the county, not recognising the fact that the population is very sparse on those hills and chiefly concentrated on the sheltered plain and in the valleys. Moreover the colouring of the maps informs us that this same county generally has an unfavourable position as well with respect of heart disease and dropsy as of phthisis; and that although rich in streams and wide valleys or plains, subject to inundation, it is singularly below the average in regard to cancer. In districts wanting in bleak hills to account for an excessive ratio of deaths from phthisis, the existence of cold, damp, clayey valleys is assumed; whilst the presence of such elsewhere where there is a minus mortality is ignored. And, speaking generally of the author's arguments, too great stress is laid upon exposure to winds, and too little on immediately surrounding conditions of soil, land-drainage, social conditions, occupations and race.

Now knowledge otherwise derived assures us that occupations and modes of life exercise a chief influence in the production of phthisis and of the wasting forms of lung-disease frequently referred to it. Improper and insufficient food, misery and overwork, overcrowding and wretched abodes, are accountable for numerous deaths from consumption quite independently of geographical conditions, and the map Mr. Haviland presents us to show the distributions of phthisis in London speaks to this fact. Still more are certain occupations chargeable with the development of consumption, and especially such as are attended by the production and inhalation of dust. To the operation of these rather than to the geographical position should we attribute the deep blue regions tinted in the map to indicate the high mortality from phthisis in the textile manufacturing districts. And with regard to several of the more phthisical regions in Wales we apprehend that the employment of the inhabitants in mining, coupled with unfavourable social conditions, is a more potent cause than their topographical features.

There is no doubt that race has something to do with proclivity to phthisis, but it comes upon us as a novelty that the Welch are so prone to the disease as the map intimates.

Dr. Richardson, in his recent work on "Diseases of Modern Life" shows that, in his experience in London, those of Saxon origin are very much more the subjects of pulmonary consumption than the Celtic race. The statistics from which this inference is drawn are certainly wanting in many col-

lateral details we need not here specify to afford them necessary precision. But, so far as they go, they are adverse to the conclusion that the Welch are a peculiarly consumptive race. The amount of blue region in the Eastern Counties rivals that in Wales, where there exist a totally distinct race and geographical features precisely the reverse of those found in the Principality.

We have necessarily accepted, in our remarks on Mr. Haviland's work, the correctness of the calculations of the ratio of prevalence of the several diseases examined, as made from the mortality tables of the Registrar-General, and of the pictorial representations conveyed by the maps. At the same time we are surprised at some of the results represented. For instance, the pottery towns of North Staffordshire are coloured to show the lowest of the three degrees of mortality from phthisis above the average, and to be on a par with the adjoining districts of Stone and Cheadle, and lower than that of Newcastle-under-Lyme, which is not a manufacturing town. This circumstance astonishes us, because, as is well known, the pottery towns have been the subject, on one or two occasions, of a special commission of inquiry on the part of the Government, by reason of the high rate of mortality there prevailing from consumption; and because, as is equally well known, potters are peculiarly subject to consumption as a consequence of the inhalation of dust in carrying on their work. To put the matter to the test, we take up the report of the causes of death, &c., issued by the Registrar-General, for the ten years 1861-70, and we find that, in Wolstanton and Stoke, the two registration districts in which the pottery towns are situated, there were respectively 31,664, and 40,219 inhabitants of all ages, and 570 and 1250 deaths registered as occurring from phthisis, being in the ratio 1·8 and 3·1 per cent. respectively. From these figures it appears that the mean mortality from phthisis, on the whole population of the two districts, equalled 2·5 per cent. Turning to Newcastle, we find a population of 13,909, and deaths from phthisis 273, which is 1·9 per cent.; whilst at Stone and Cheadle, leaving out figures, we find the percentage respectively to be 2·3 and 1·8. Lastly, taking the neighbouring district of Leek, which is shaded in the map the deepest blue, we find the mortality from phthisis not to surpass that of Stone viz. 2·3 per cent.

These calculations indicate some error in the colouring of the map, for Newcastle should not have a deeper tint than the potteries and Stone, but the contrary; and Leek, instead of being the darkest blue, should have the same shade as Stone. It does not matter at all, in estimating these rates in connection with

geographical position, that we have quoted figures for the decenniad following that examined by Mr. Haviland; but the returns before us indicate how greatly results would be influenced if we could get accurate records of the causes of death. For example, although only 570 deaths are assigned to phthisis from Wolstanton and 1250 from Stoke, it is important to note that 1178 deaths in the former district and 2025 in the latter are attributed to "diseases of the chest," a category including a very considerable proportion of cases of phthisis, and especially of the form prevalent among potters.

A concluding section (p. 111) of the author's work, on the "Mode of Investigation by Medical Officers of Health," illustrates the benefits likely to accrue from minute examination of districts separately, and it likewise demolishes, in no small degree, the wide conclusions contained in the treatise at large. Mr. Haviland recounts the results of his inquiry at Hardingsstone, as to whether "such a disease as phthisis depends upon social or climatic causes or both combined," and the conclusions he was driven to are, that the climate, including the wind, cannot be considered an exciting cause, and that soil-dampness will not account for its prevalence. Having eliminated these morbid agencies, he falls back upon "overcrowding and dilapidated dwellings" as the principal source of mischief, and enunciates as a general truth,

"That, however much the distribution of diseases may appear to be regulated by the climate, or by the physical and geological characters of the country, they do so only according to well-known laws in many cases. For instance, if the disease lie in the lungs, these organs are weakened, rendered irritable, and susceptible of the influence of harsh winds—winds, in fact, that are characterised by their dynamical element, and which prematurely ripen the latent disease seeds" (p. 113).

We cannot conclude without calling attention to an important omission made in discussing the question of phthisis and its geographical distribution, viz. in not noting that, though in London the mortality from the malady among females be the lowest among the several registration divisions of the kingdom, the mortality from it amongst males is very considerably the highest. This fact alone suffices to prove that it is not geographical peculiarities that are most concerned in producing such mortality, but social conditions obtaining in the population.

Here we must stop, having already bestowed much more time and space upon the work before us than we intended when setting out on its review. The subject, almost a novel one, and treated in a novel fashion, has awakened an interest in it,

whilst the various inferences and statements advanced by the author in successive pages have impelled us to many criticisms and observations which a cursory consideration would have saved. Our final impression of the work is that it is a most painstaking one, but that its basis is unsubstantial and its conclusions generally untenable.

### VII. Recent Physiological Works.<sup>1</sup>

THERE are probably few greater tasks that a man can set himself at the present day than that of writing a text-book on physiology. To do it well requires an unusual combination of powers. He who undertakes it must be a good chemist, for a knowledge of chemistry is indispensable to one who has to determine the composition of the different secretions, and to trace the changes that are produced in them by direct or indirect excitation, to analyse the composition of both the fluid and the solid parts of the body, to follow the changes effected in them by respiration, and to pursue the alterations undergone by the food from the moment of its injection to the period when reduced to its simplest compounds it is discharged from the body. He should be a good botanist, for how otherwise can he possess a sufficiently wide knowledge of those processes which are only intelligible when they are seen reduced to their simplest expression in plants. He cannot afford to be ignorant of something more than the elements of physics, since mechanical, thermal, and electrical questions are constantly rising whatever may be the subject under discussion. He must be familiar with the numerous and often highly complicated apparatus required to exhibit many of the most interesting results of modern physiological research. He must be an accomplished microscopist, for structure and function are so intimately connected that no perfect knowledge can be ob-

<sup>1</sup> 1. *Elements of Human Physiology*. By D. L. HERMANN, Professor of Physiology in the University of Zürich. Translated from the fifth edition by ARTHUR GAMGEE, M.D., F.R.S., Brackenbury, Professor of Physiology and Histology in Owen's College, Manchester; Examiner in Physiology in the University of Edinburgh. London, 1875.

2. *Nouveaux Éléments de Physiologie Humaine comprenant les Principes de la Physiologie Comparée, et de la Physiologie Générale*. Par H. BEAUNIS, Prof. de Physiologie à la Faculté de Médecine de Nancy. Paris, 1876.

3. *A Text-book of Human Physiology*. By AUSTIN FLINT, junr., M.D., Professor of Physiology and Physiological Anatomy in the Bellevue Hospital Medical College, New York. Illustrated by three lithographic plates and 318 woodcuts. London,

tained of the one without the other. If we add to these requirements a considerable amount of literary skill to place his knowledge in a condensed and agreeable form before his readers, we shall have a man that is not often seen.

Very cursory inspection of any of the modern text-books will show that the advances made in physiology, like those in chemistry, during recent years have been in great measure dependent on the adoption and application of the various improvements and developments of physical science to the elucidation of obscure problems. The great perfection to which the microscope and the processes of staining and mounting have been brought, a better knowledge of the action of electricity and of variations of temperature, and the employment of a great variety of mechanical apparatus, amongst which those of Volkmann, Ludwig, Weber, Czermak, Chauveau, Bernard, Pflüger, Cyon and M. Marey, deserve especial notice and commendation, has elicited many new facts and greatly extended, as well as rendered more accurate, our knowledge of the structure and functions of all the tissues and organs of the body.

Take blood, for example. Commencing with the extremely accurate micrometry of the corpuscles, by Gulliver, which has scarcely received the credit it deserves even in this country, and still less in foreign works, we have gained in succession a knowledge of the number of the corpuscles, of the action of various reagents, as heat, electricity, chloroform, and various gases, upon them; the discovery of blood-crystals; the separation of the colouring matter of the blood; its spectrum analysis; the nature and proportion of the gases, and their state of combination; the movements and division of the white corpuscles; and a score of other facts, which have all resulted from the application of improved chemical or physical knowledge, and of greater precision in the use of instruments.

So, too, with respiration, the complicated apparatuses of Regnault and Reiset, and of Pettenkofer, for determining the changes in the air, the classical researches of Bert, and the graphic representation of the respiratory movements by means of registering levers and drums, have added immensely to the extent and accuracy of our knowledge of this important function.

The progress in chemistry has in like manner, exerted a powerful influence and thrown much light, though great hiatuses still exist, upon the large subject of the statics of nutrition. And Helmholtz' singularly interesting researches on sound, have been of much service in elucidating the conditions of phonation and audition.

In comparing the treatises at the head of this paper we would remark that that of Beaunis is more comprehensive in its scope than that of Hermann, whilst the latter is more philosophical in its tone. The former is more of a compilation, the latter is more original. The former is, perhaps, better adapted for the student than the latter, which is more appropriate for the master. Beaunis gives not only an account of the results of certain experiments, but describes and often adds an illustration of the instruments required, and shows how they are to be used. Hermann avoids all details, and only records results in the briefest possible manner. He gives the cream of a long paper in many instances in a couple of lines, and this method, whilst it renders the work very valuable as a book of reference, undoubtedly lends it a dry and somewhat unreadable character—a very little of it goes a long way. It belongs to the class of books that Lord Bacon described as requiring to be digested, and it is of rather tough digestion, too, though when absorbed it will be found to constitute wholesome nourishment enough. Dr. Flint's large treatise is, in our opinion, better adapted to the use of the English student than either of the other two. It is very full, perhaps, rather too full considering the time allotted for its perusal, but it is written in a remarkably easy style, and enters into details that are very cursorily given in the other works.

M. Beaunis's work contains several original features. The first is a chapter entitled the physiological laboratory, in which the general arrangement and fittings of such an institution are given in detail. In many instances, drawings of the instruments are given, as, for example, in the case of the polygraph of Marey with the registering cylinders for tracings; the chamber for exposure to various degrees of temperature; the primary and secondary coils of Dubois Reymond, Schlösing's regulator, Pulvermacher's forceps, the lever key of Dubois Reymond, Ruhmkorff's commutator, &c. This chapter also is succeeded by a very useful one to the experimenter, on the anatomy of the frog, in which full-page illustrations are given representing the dorsal and ventral aspect of the bones and muscles, and of the circulatory and nervous apparatus. Then, at the head of each chapter, in imitation of what is commonly done in anatomical treatises, a paragraph is given in small text, noting the proceedings and instruments required. Thus, to quote the very first we happen to turn to, we find, under the head of *Sac pancreaticus*, the following serviceable résumé:—

"*Proceedings to obtain the pancreatic juice.* Pancreatic fistulæ (Regnier de Graaf, 1662).—To establish a pancreatic fistula, large and vigorous animals are selected by preference, such as shepherds'

dogs and cattle. 1. The dog. Proceeding of Claude Bernard. The animal is placed on its left side, and an incision about five inches in length is made in the right hypochondrium, below the border of the ribs, through which the duodenum and pancreas are drawn out. The largest pancreatic duct that opens separately into the duodenum is rapidly isolated (a figure is here introduced, showing the disposition of the pancreatic duct of the dog), being recognised by its whitish pearly colour. This duct is opened with a pair of fine scissors, when a large drop or two of a colourless fluid will escape. The canula is at once introduced and retained in position by a thread of silk, which is allowed to hang out of the abdominal wound, and this is immediately closed by suture. The canula falls out in the course of a few days and the wound cicatrises, so that by this means only a *temporary* fistula is produced. (Here a second figure shows a mongrel terrier with such a fistula.) 2. Proceeding of Ludwig and Weinmann. These experimenters have endeavoured to obtain a *permanent* fistula. To prevent the obliteration of the pancreatic duct they introduce a piece of lead wire, which is attached to the suture of the abdominal wound. The walls of this duct then adhere to the cicatrix, and a canula is placed in the orifice. 3. Ox. Proceeding recommended by G. Colin. The pancreatic duct often separated from the gland by an interval of 2 or 3 centimetres is capable of receiving a canula 8 or 9 millimetres in diameter ( $\frac{1}{4}$  inch). In the right flank an incision should be made parallel to the last rib, and the peritoneum being divided, the pancreatic duct appears between the duodenum and the lower extremity of the gland. It is cut through and a glass tube introduced. The abdominal wound, through which the glass tube passes is then closed. (A third figure is here inserted representing a bull with a pancreatic fistula and its collecting apparatus.) 3. Pig. Pancreatic fistulæ are readily established in this animal by a similar proceeding. In the sheep, the goat, and especially in the horse, it is, on the contrary, extremely difficult to make such fistulæ and the best mode of obtaining the pancreatic juice of these animals is to kill them when in full digestion.

*"Artificial pancreatic juice.*—Bruise the gland, cut up into fragments, in a mortar, with some oil (Claude Bernard); or, better, make a watery extract; or, better still, make an extract with glycerine in accordance with the method suggested by von Wittich. The period chosen should be when the gland is pink, and in a state of activity.

*"Operations on the pancreas.*—1. Extirpation of the pancreas. This operation has been attempted by Brunner and by Claude Bernard, but the animals usually die from peritonitis. Birds, however, according to Schiff and Colin, survive the operation. 2. Ligation of the pancreatic ducts. The same proceeding is required for this operation as for the formation of a pancreatic fistula. The continuity of the tube is re-established after a certain interval. 3. Destruction of the pancreas. The injection of oil into the pancreas is followed by its dissolution, but the animals die (Claude Bernard).



Schiff, instead of oil, injected paraffin, and found that the gland became transformed into a hard mass, and that the animals bore the operation very well."

All this we think is useful and shows sufficiently well to the ordinary student what the method of physiological investigation is. It is not in any way referred to in Hermann's work. But, as illustrating the observations we have just made in regard to the terseness of Hermann, we may extract the final remarks he makes upon the physiology of the secretion of the pancreas—

"The nerves affecting this secretion are unknown. They appear to be called into action reflexly by stimulation of the gastric mucous membrane, just as are those of the salivary glands on stimulation of the mucous membrane of the mouth (Ludwig), hence the secretion of the gastric and pancreatic juices occurs simultaneously (Bidder and Schmidt). Irritation of the medulla oblongata increases the flow probably only by inducing contraction of the duct (Landau). Irritation of the central end of the vagus stops the secretion (N. O. Bernstein). The same stoppage occurs during vomiting (Weinmann, Bernard). The relative amount of solid constituents is inversely proportioned to the rapidity of secretion (Weinmann); that of salts, however, is pretty constant, and is the same as in blood serum (N. O. Bernstein)."

Dr. Flint gives drawings of the pancreas in relation to the intestines and bile ducts, of the canula used for pancreatic fistula, and of the mode in which it is inserted and worn by the animal, and describes its action very well, but he makes no mention of its being under nervous influence at all. And it is under such a heading that the contrast between many English and foreign works may be observed.

A feature characteristic of M. Beaunis' work is, that he has introduced a short chapter on physiological toxicology, in which is discussed the action of the anæsthetics, of curara, and of the principal poisons employed in physiological experimentation.

It may be interesting to compare the order in which such a subject as muscle is considered by the three authors, and we append the heads of the chapters and section upon it in each.

#### HERMANN.

Structure of *striated* muscle. Chemical constituents. Mechanical condition and chemistry of—(1) Muscle at rest; (2) Muscle in rigor; (3) Muscle in activity. Stimuli for muscle. Irritability of muscle. Theory of muscular activity. Changes in the form of contracting muscles. On the amount of work done by contracting muscle—(1) When the transformation of energy within the muscle is at its maximum; (2) When the activity is not at its maximum. Thermic and electric phenomena of muscle. Inter-dependence of the phenomena of muscle and theories of muscular activity. Smooth muscles. Contracted cells—protoplasmic movements. Ciliated cells and spermatozoa. Uses of muscles and mechanism of the skeleton. Conditions of equilibrium and of active locomotion of the whole body.

## BEAUNIS.

Structure of muscle. Physical properties. Tonicity. Physiological properties of muscular tissue. Irritability of muscle. Muscular contraction. Myography, with an account of the various forms of apparatus in use for this purpose. Of muscular work. Of muscular fatigue. Physical condition accompanying muscular contraction. Sound. Heat, &c. Theory of muscular contraction. Cadaveric rigidity. Smooth muscle.

## FLINT.

Movements (amœboid and ciliary). Movements (due to elasticity). Movements (muscular). Physiological anatomy of the involuntary muscles. Contraction of involuntary muscular tissue. Physiological anatomy of the voluntary muscles (with a very indifferent illustration, though taken from a photograph). Accessory tissues, composing muscle. Physiological properties of the muscles—elasticity, tonicity, sensibility, contractility or irritability, muscular contraction, mechanism of prolonged muscular contraction. Electric phenomena. Muscular effort.

The histology of muscle is given somewhat briefly by Hermann, who states in the text that the appearance of transverse striæ is caused by the presence of rows of little bodies (disdiacasts of Brücke) possessing more strongly refractile properties than the ground substance. He relegates to small type without entering into details the views of Hensen, Krause, Flögel, Merkel and Engelmann, and he gives no drawings.

Beaunis also is contented with Bowman and Brücke's views, but gives the complicated scheme of Engelmann with a drawing. Schäfer's views are not referred to, though they have been published long enough to allow of their having been transmitted to Nancy. Hermann states that the majority of the muscle tubes run the whole length of the muscle, though some end by pointed extremities in the interior of the muscle. Beaunis following Rollett more implicitly observes that they do *not*, as a rule, run from one end of a muscle to the other, unless the muscle be very short. Both authors admit the direct continuity of the nerve and muscle tissue. The ultimate branches of the nerves penetrating the sarcolemma and coming into contact after losing their medullary sheath with the contractile substance.

The account in Dalton is also rather bald and in places inaccurate. For example, he says that "a primitive muscular fasciculus runs the entire length of the muscle." Yet it is now very generally admitted that they are rarely more than one or two inches in length. And, again, the sarcolemma or "sheath contains the true muscular substance, and is not penetrated by blood-vessels, nerves, or lymphatics." The evidence in favour of the penetration of the sarcolemma by the axis cylinders of the nerves is to our minds overwhelming. He says little or nothing in regard to the more recent views of the structure of muscle.

Hermann quoting from Tergast observes that for every nerve-fibre which enters one of the muscles of the eye there are

from eight to ten muscular fibres, whilst in other muscles there are as many as twenty to eighty. The chemical composition of muscle has not been, he says, quite satisfactorily made out.

The superiority of Hermann's treatise in all matters of theory to Beaunis and Flint is sufficiently evidenced by the following abbreviated account of the 'Theory of Muscular Activity,' which will be interesting to our readers as summing up in a very intelligible manner, much of the work that has been done in this direction of late years in Germany, and which would require the perusal of many special treatises and memoirs.

After observing that "The chemical actions which take place during contraction and rigor of muscles are most probably identical (Hermann)," and giving various reasons in support of this view, Hermann proceeds to say, that—

"The following may in all probability be regarded as the simplest theory of the chemical processes occurring during contraction and rigor; muscle contains at any moment a store of a complex nitrogenous substance dissolved in the contents of the muscle-tubes and in the muscle plasma, which may be described for the sake of brevity as the 'energy generating,' or 'inogene substance.' This inogene substance is capable of undergoing a decomposition in which energy is evolved and the following products yielded, viz., carbonic acid, sarcolactic acid, probably glycerine, phosphoric acid, and a gelatinous body (myosin) of an albuminous nature, which separates spontaneously and afterwards contracts firmly, becoming probably concentrated. This decomposition occurs spontaneously but slowly while the muscle is at rest, the rapidity of its occurrence being determined by the height of the temperature. It takes place instantaneously at the temperature of heat rigor. It is, moreover at once accelerated by stimulation, and this acceleration is essentially what occurs during the active condition. When the substance is entirely used up, muscular activity is no longer possible.

"The 'inogene substance' has not hitherto been isolated, as in every method of chemical investigation yet devised the characteristic decomposition occurs. The latter may, indeed, be prevented by subjecting the muscle suddenly to a strong heat (scalding) or to the action of mineral acids, but both these methods destroy the substance. As regards its composition it would seem to resemble hæmoglobin, as both yield an albuminous body on decomposition. On account of the analogy between the chemical processes of muscular activity and of rigor we must suppose an expenditure of glycogen (O. Nasse) to occur during the former as during the latter.

"As the essential energy-yielding substance is used up during muscular exertion, a continued renewal of it is necessary in order

that a muscle may retain its active properties. This is effected, as already mentioned, both after contraction and rigor of the blood. The blood effects the recovery of the muscle, not only by the production or renewal of the 'inogene substance,' but also by the removal of the products of decomposition themselves, which are harmful to the muscle. Blood removes from muscle carbonic acid and most probably also sarco-lactic acid (du Bois-Reymond) both deleterious substances, and it gives up to that tissue oxygen. It is, however, evident that oxygen alone cannot repair all the losses sustained by muscle, as carbon and hydrogen are continually leaving it in the form of carbonic acid and lactic acids. The blood must, therefore, supply to it, besides oxygen, organic materials containing carbon and hydrogen.

"The whole of the products of the decomposition of 'inogene substance' do not leave the muscle, for as the excretion of nitrogenous material is not increased by muscular exertion, it must be concluded that the myosin remains within the muscle. In consequence of this, and of the fact that it is not the prepared substance, but only the materials necessary to form it, which are conveyed to the muscle by the blood, it is most probable that the recovery of muscle after exhaustion, apart from the mere removal of the effete materials consists in a synthesis of the 'inogene substance,' in which myosin plays a part, and for which the blood supplies oxygen and some non-nitrogenous organic body hitherto undiscovered (Hermann). Myosin, therefore, according to this theory, undergoes in muscle a complete cycle of chemical changes.

"The following conditions are necessary for the restoration of muscle:—1. The addition of oxygen which may take place, though to a limited extent, in excised muscles. 2. The addition of the yet unknown organic body referred to above, of which it is possible that excised muscles may contain a slight store. The latter supposition may at least be taken to explain the facts that restoration is to a certain extent possible by simple exposure to the air, and that irritability is retained for a longer time when the surrounding atmosphere contains oxygen. 3. The presence of myosin capable of being used, *i. e.*, which has not passed into the firmly contracted condition. A consideration of this third condition will explain why the restoration of muscular tissue by means of the circulation is limited. The synthesis of the 'inogene substance' in which oxidation is one of the processes seems to be analogous to the synthesis of hæmoglobin, in which oxygen also plays a part.

"The chemical process, which forms the basis of muscular activity and the process of restoration, occur without any essential dependence, one upon the other; it follows, therefore that the excretion of carbonic acid by the muscles and by the whole organism, which is characteristic of the former process, and the absorption of oxygen, muscular and general, which is an important feature in the latter, are equally independent. It must, however, not be forgotten that whenever the decomposition of 'inogene substance' is accelerated, as during muscular activity, the processes of restoration and repair are

also increased, *i. e.*, that muscle takes up more oxygen from the blood during activity than during repose; for it is in this way that the danger of exhaustion is diminished. This regulating action is explained chiefly by the fact that the circulation is accelerated in a muscle by its contraction (Ludwig and Sczelkow). If the exertion be excessive the repair of the 'inogene substance' cannot keep pace with its expenditure and the muscle becomes for the time acid, and can only be stimulated to contraction with difficulty. This condition is called exhaustion and resembles the state of approaching rigor, such as is induced by subjecting muscle to heat. Certain circumstances render it probable that individual fibres of a muscle may pass into a condition of complete rigor, especially if the previous exertion of the muscle have been excessive. In such cases the myosin is no longer capable of use in the synthetic process of restoration, and the loss thus occasioned must be repaired by the formation of 'inogene substance' in some other manner. There must therefore occur, in addition to the above-mentioned functional exchange of matter, another process which may be described as the material exchange due to wear and tear. We have only now to suppose that the myosin of the completely coagulated fibres decomposes further with the formation of creatine, the separation of which would serve to increase the amount of urea excreted and perhaps also of fat, as fibres which have undergone fatty degeneration are found in every muscle. This theory would serve to explain the statements of the occurrence of increased nitrogenous excretions, &c., after muscular activity. Under such circumstances other albuminous bodies resembling myosin which are held stored up in the muscle would be used instead of the latter in the synthesis of 'inogene substance.' Such albuminous constituents of muscle are those which coagulate between 40° and 60° C."

Such is Hermann's theory of muscular activity, which we think very fully and satisfactorily accounts for the phenomena observed.

If we now turn to the corresponding section in M. Beaunis' book we find, in our opinion, a much less complete and satisfactory explanation.

"What," he asks, "is the real nature of muscular contraction? The question is indeed, at present, far from being decided, and it is difficult to choose between the conflicting theories that have been broached upon the subject. The theories of muscular contraction may be reduced to three groups—the physical theory of elasticity, the mechanical theory, and the chemical theory.

"A. Physical theories of elasticity.—1. Theory of Ed. Weber. Ed. Weber, who is followed by the greater number of physiologists, and amongst others by Küss and Volkmann, maintains that muscular contractility is only a form of elasticity. Muscle has two natural or normal forces; a natural form (No. I of Küss) in which it is in a state of repose, and a natural form (No. II. of Küss) in which it is contracted. What is commonly called the passage from the state of rest or repose to contraction is only the passage from

No. 1 to No. 2, but the muscle is not more active under this form than under the first, since in both cases it exerts a certain force of traction on its two points of attachments. The stimulus only changes the elastic force of the muscle as heat changes that of a bar of metal. In regard to the cause of this change of elasticity Volkmann supposes the nervous irritation produces chemical actions in the muscle which modify the equilibrium of the molecules. The theoretical reasons that Volkmann has recently advanced in support of this theory appear to me to be insufficient.

2. The theory of Rouget. Rouget also attributes muscular contraction to elasticity, but he understands this elasticity quite differently from Weber. According to him muscular fibre is comparable to the stalk of the *Vorticella*, that is to say, to the specially contractile pedicle by which that infusory animalcule attaches itself to foreign bodies; in the ordinary state this style is elongated and forms a scarcely perceptible spiral, but as soon as any stimulus is applied the elongated spiral becomes suddenly shortened to four-fifths and forms a helicoid spring with closely compressed coils; it is this last form which the stalk takes after the death of the animal. The state of activity associated with life and with the continuity of nutrition corresponds on the contrary to the suppression of the phenomena of nutrition, and it is a pure affair of physical elasticity; the stalk being no longer distended by the circulation of nutritious fluids returns to its natural form of spiral elastic spring. It is the same with muscular fibre. During life it tends unceasingly to contract in virtue of its elasticity; but this tendency to shortening is antagonised by a tendency to elongation due to the nutrition of the muscle itself, and probably to the production of heat, of which it is the cause. Everything that interferes with the work of nutrition (nervous irritation, ligature of the vessels supplying the muscles) causes this tendency to elongation to disappear, and the elasticity alone acting contraction takes place. The augmentation of heat of the muscle at the instant of its contraction is explicable on the supposition that the heat which was employed in extending the muscles becomes free at the moment when the muscle shortens.

"B. *Mechanical Theories*.—The modern theories of the correlation of physical forces soon suggested their application to muscular movements. R. Mayer considered muscle as a sort of machine comparable to a steam-engine producing heat and mechanical work. In the state of repose heat alone is produced; in the state of activity it produces more heat, but a part of this heat is converted into movement. I. Béclard also holds this view, and has made some interesting experiments in support of it. But this production of heat is itself associated with chemical phenomena, and the mechanical theory attaches itself strongly by one point to the chemical theories. C. Voit, on the contrary, denies the possibility of the transformation of heat into movement in the organism, and believes in a transformation of muscular electricity into heat and motion. Prévost and Dumas had already advanced the opinion that muscular contraction proceeded from electricity, but apart from the negative

variation of the muscular current in contraction, no experiment has been made to confirm this theory, at least, if the following experiment of C. B. Radcliffe be not admitted. This observer places a leaf of caoutchouc between two plates of gold; he charges the superior metallic plate with electricity and then finds that the leaf of caoutchouc becomes extended. When the apparatus is discharged the caoutchouc leaf retracts. Radcliffe proceeds from this to compare muscle when in a state of repose to a Leyden phial charged, whilst it contracts as soon as the discharge has taken place.

"c. *Chemical theories.*—Two secondary theories are connected with the chemical theories of contraction, that of oxydation and that of disintegration:—

"1. *Theory of oxydation.*—In this opinion oxydation constitutes the true cause of muscular movement. This opinion has in its favour the known fact of muscular respiration, respiration which is more active at the moment of contraction; but it has already been shown that the elimination of carbonic acid is not quite in relation with the absorption of oxygen. It is true that the surplus of oxygen may exist in a certain quantity of water formed in the muscle and impossible to estimate. Moreover, what elements would this oxydation affect? The substance of muscles themselves, or the oxidisable materials brought by the blood? The azotised, or the non-azotised principles. Are creatine, creatinine, and the other nitrogenised compounds, the products of simple nutritive oxydation, or of oxydation taking place during muscular contraction? or, rather, as appears to be indicated by the production of lactic and carbonic acids by and in muscles in action, do not the muscles use up in their contraction only non-azotised materials. It is a question whether this last hypothesis is in accord with experiments that will be subsequently detailed.

"2. *Theory of decomposition.*—Starting from the fact that muscular contraction is capable of taking place without oxygen and that lactic and carbonic acids continue to be formed, notwithstanding the absence of oxygen, Hermann does not admit the occurrence of oxydation, but only of decomposition. The muscle, according to this observer, contains a store of inogene (a substance not yet isolated) containing nitrogen capable of undergoing decomposition, whilst giving off energy into myosine, lactic, and carbonic acids. The blood carries away the two latter from the muscle, but leaves the myosine, whilst it brings to it oxygen and anonazotised substance (yet unknown) which, with myosine, reforms the inogene substance. This theory cannot be admitted until inogene and the non-azotised substance have been isolated.

"*En résumé* muscle is the seat of chemical phenomena, of the production of heat and of the production of movement, and it is certain that a close tie exists between these three phenomena, the laws of which are however still unknown."

From these somewhat long extracts a tolerable estimate may

be drawn of the literary ability of the two authors, and we may now append a few observations that have suggested themselves in reading other parts of their respective treatises.

In the third chapter of M. Beaunis' work, which treats of the 'Liquids of the Body,' we find a fair account of the blood. In this a method is given we have not elsewhere seen noticed, by which the plasma may be separated from the globules, and which consists in receiving the blood in a tube surrounded by ice, made to rotate rapidly in a horizontal direction. The plasma, it is said, separates from the corpuscles in the course of a few minutes. The following numbers are given: "There are according to Vierordt, five millions of corpuscles in one *millimetre* (cubic). Hoppe-Seyler finds that in 1000 parts of blood, 326 parts are constituted by the corpuscles, and Welcker gives a total oxidizable surface for the whole of the corpuscles of 2816 square metres." Beaunis gives their density at 1.105, which is rather high, and considers they are "semi-solid, *homogeneous*, and destitute of membrane and nucleus," yet immediately afterwards accepting Brücke's view, remarks that "The blood-globules is composed of two parts the *stroma* or globular mass, and the *colouring-matter* or hæmoglobin," and proceeds to describe how they may be separated. The stroma, he observes, is again composed of paraglobulin and of protagon, with salts, chiefly of potash. In regard to the coagulation of the blood various theories are given, but none are regarded as satisfactory.

The general physiology of nutrition is very well and clearly given by Beaunis, but we think still more intelligibly by Hermann, who is much less given to tables of composition than Beaunis. In both works the Physiology of Generation is indifferently and very shortly given. Beaunis has, however, published another treatise upon this subject, and refers his readers to that volume.

No subject has been more laboriously investigated of late years than the physiology of the nerves, yet the practical results have hardly been commensurate with the amount of time and labour that have been expended upon them. The application of electrical currents for the relief of pain and for the restoration of mobility in paralysed nerves and muscles, is still, notwithstanding the numerous experiments and large clinical experience of Duchenne, Ziemssen, Erb, and others, very empirical. It cannot as yet be definitely stated whether the greatest amount of benefit in a given case will certainly be obtained from the application of the anode or of the cathode to the proximal or distal parts of the nerve; whether both or one should be moved over the part affected, or kept stationary; whether the



current should be divided as with a metallic brush or applied at a single point, and in many instances, though certainly not in all, it cannot be predicted whether galvanic or interrupted currents will be most effective. The discovery of electrotonus and of its laws by Pflüger has done little to aid practitioners in their daily work.

The main facts in regard to the physiology of the nerves that have been ascertained of late years are, however, given very well by both authors. There can be no doubt that the axis cylinder which was formerly regarded as simple, is as Beale and Max Schultz have showed a *fasciculus* of much finer fibrils, and Ranvier's statement that the fibres exhibit contractions at certain points at tolerably equal distances, indicating the limits of the original cells from which the nerves were developed seems to be generally admitted. The difficulty that formerly existed in regard to the difference between a centrifugally and a centripetally conducting nerve is now overcome by supposing that all nerves can conduct in both directions, but that only one of the end-organs (centric sensory cell or peripherically-lying muscle) of each is capable of indicating that conduction. The rapidity with which conduction of motor impulses is effected in the frog is about twenty-eight yards per second. In man, Helmholtz finds that the rapidity of motor impulses is about thirty-six yards per second, and Hermann thinks that this number expresses tolerably accurately the rate of conduction of sensory impulses. The rate of conduction in the motor nerves of man is determined by tracing upon a myograph the curve of contraction of the muscles of the thumb by means of their increase of thickness (Hermann). Two tracings must be taken, one when the stimulus has been applied to the nerve at a point on the arm near to the hand, and the other when it has been applied at a point more remote from the hand from which the determination is easily made. The phenomena and conditions of electrotonus may be regarded as fairly intelligible after the careful researches of Dubois Reymond, Eckhard, Pflüger and Hermann. Electrotonus is that altered condition that is established throughout the whole length of a nerve when a constant current, called the polarising current, is made to pass through any segment of it. That portion near the negative pole is found to be in a state of increased excitability or *cathelectrotonus*, whilst that portion near the anode or positive pole is in a state of *anelectrotonus* or of diminished excitability. When a nerve, still in continuity with muscle, is stimulated by such a constant current, the excitation producing muscular contraction proceeds from the cathode on closing, and from the anode on opening, the stimulating current. The

results, however, differ in accordance with the strength of the current. The natural current of nerve and muscle is very simply explained by Hermann as an effect of contact, the contents of nerve tubes which are dying, or in process of oxidation being negative to the contents of nerve-tubes that are living and at rest. This is far simpler than the theory of electromotive molecules of Dubois Reymond, and is probably correct.

Of all the special nerves none have been more investigated than the pneumogastric, and we shall conclude this article by an extract from Hermann's treatise which shows well his great powers of condensation. After stating that the vagus contains—1. *Centrifugal* fibres, viz., *a*, motor; *b*, inhibitory for the head; *c*, secretory for the stomach and kidneys; and, *d*, vaso-motor for the lungs; and 2, *Centripetal*, viz., *a*, sensory fibres for the respiratory, gastric, and cardiac apparatus; and, *b*, regulating fibres, to wit, accelerating for the respiratory centre; inhibitory for the same centre; stimulant for the cardiac inhibitory centre; stimulant for the vaso-motor centre (pressor fibres); inhibitory for the same centre; stimulant for the salivary secretion; inhibitory for the pancreatic; and stimulant for the sugar formation in the liver; he adds—

The results of experimental section and irritation of the vagus and accessory, which have served to determine the various series of fibres contained in those nerves will now be gathered into a *résumé*.

1. Section of the spinal accessory above its point of union with the vagus (or, as is the usual plan, removal of the accessorius-roots from the cord), paralysis of all the muscles dependent upon the vago-accessorius for their nerve-supply; according to some, the laryngeal muscles (van Kempen, Navratil) are not affected, and the power of swallowing is not entirely lost. In addition, section of the accessorius causes acceleration of the heart's action, while stimulation produces slowing (Waller, Heidenhain), unilateral paralysis of the external portion of the accessorius causes a twisting of the head.

2. Irritation of the vagus above the point of union with the accessorius causes among other things, contractions in larynx, pharynx, and œsophagus.

3. Section of the vagus trunk in the neck causes (*a*) paralysis of laryngeal muscles and, in consequence, when both vagi are divided, inaction of the vocal cords, loss of voice and passage of portions of food into the lungs, whereby fatal pneumonia is induced; (*b*) quickening of the heart's action; (*c*) slowing of the movements of inspiration; (*d*) prevention of those reflex acts which stimulations applied to larynx, pharynx, and stomach, normally induce; (*e*) prevention of the last act

in the operation of swallowing, so that the œsophagus becomes filled with food ; (f) interruption of the sugar formation in the liver (?).

4. Irritation of the peripheral portion of the divided vagus in the neck causes (a) spasms of the glottis or contraction of the laryngeal muscles, which is also induced by irritation of the peripheral end of the inferior of the laryngeal nerve ; (b) slowing of the heart's action, and finally stand-still of that organ in diastole ; (c) contraction (so it is said) of the smooth muscles of the bronchi, thus narrowing their lumen somewhat ; this has, however, been frequently denied by Donders, Wintrich, Rosenthal, and Rügenberg, but has been again supported recently by Schiff ; (d) contraction of the stomach, intestine (?), uterus (?), &c. ; (e) increased renal secretion (?).

5. Irritation of the central portion of the divided vagus in the neck causes (a) quickening of inspiratory movements which proceeds even to inspiratory tetanus ; occasionally, however, an opposite result follows (p. 168) ; (b) increased sugar formation (?) ; (c) increased salivary secretion (?) ; (d) diminished pancreatic secretion ; (e) diminished blood-pressure if stimulation be applied above the point of union of depressor and vagus ; (f) slowing of the heart's heat when the other vagus is intact.

6. Section on paralysis of the inferior laryngeal nerve and paralysis of the laryngeal muscles, causing the same phenomena as section of the vagus in the neck (see 3, a) ; aneurisms of the aortic arch sometimes press upon the left inferior laryngeal nerve, thus producing paralysis of the left vocal cord.

7. Section of the superior laryngeal nerve causes a slight slowing of inspiration (Sklarek), on account of the motor fibres for the larynx and especially for the crico-thyroid muscles which it contains. Navratil has recently denied that the crico-thyroid muscle receives motor supply from this nerve.

8. Irritation of the central portion of the divided superior laryngeal nerve causes (a) slowing of inspiration, which proceeds to complete cessation of the respiratory movements (Rosenthal) ; (b) increased blood-pressure by inducing contraction of the arteries.

9. Irritation of the central portion of the divided depressor branch of the vagus causes dilatation of all the arteries, and, in consequence, a fall of blood-pressure (Cyon and Ludwig).

The irritability of the fibres of the vagus or, more correctly, of the end-organs of the fibres, varies with the fibre.

On irritation of the peripheral portion of a divided vagus a stronger stimulus is needed to produce slowing of the heart than is necessary to produce contraction of the laryngeal muscles (Rutherford). During irritation of the central portion

the fibres which cause quickening of respiration are sooner exhausted than those which cause slowing (Berkart). The inhibitory fibres for the heart are sometimes very unequally divided between the two vagi.

It is right to say that the whole section on the *vagus* is extremely well given by Dr. Flint, but some parts of his treatise require working up. In the case of the blood, for example, which is the subject of the first chapter. The account of the spectrum analysis of the blood is very meagre and unsatisfactory, whilst in the section on the development of the blood-corpuscles Dr. Flint disposes of the generally received opinion in a very few words.

"In many works on physiology," he says, "we find accounts of the development of the red corpuscles from the colourless corpuscles or leucocytis, which are supposed to become disintegrated, their particles becoming developed into red corpuscles, but their seems to be no positive evidence that such a process takes place." This scarcely, we think, represents the current opinion upon the matter. It is believed that the nuclei of the white corpuscles form the red corpuscles by increasing in volume and acquiring colour, and we think this is more probable than Dr. Flint's suggestion that in the adult they are probably formed in the liquor sanguinis by the same process by which they take their origin in the ovum, *i. e.*, by genesis in the sanguineous blastema. The agency of the spleen, of the lymphatic glands, and of the cancellous tissue of the bones is altogether ignored.

We were somewhat surprised again to find no reference to the investigations of Hitzig, Ferrier, and others, on the reaction of the brain to direct stimulation. This should hardly be in a book, the preface of which bears date November, 1875. In the section on generation which, as a whole, is rather feebly written, the distinction between ephiblast, mesoblast, and hypoblast, and the account of the organs derived from them are not clearly given, though Dr. Flint has introduced figures from Brücke. The best parts of the work are those on nutrition and circulation, which are very fully and interestingly written.

The very beautiful drawings that accompany and illustrate Dr. Flint's work are deserving of special mention. Many of them are taken from Sappey's '*Anatomie descriptive*,' and are first rate—extremely accurate, and wonderful specimens of wood engravings.

The extracts we have given will, we think, enable our readers to decide for themselves as to which book will best meet their requirements.

VIII.—Diseases of the Nervous System.<sup>1</sup>

THE work of Professor Charcot here noticed is the complement of the admirable and now well-known treatise of Dr. Weir Mitchell on 'Injuries to Nerves.' The latter book is devoted to the consideration of the effects of injuries to the peripheral nerve trunks and their branches; the former treats of the effects of injuries to the great nerve centres, the brain and spinal cord. Thus, while each appropriates a separate field of research and forms an independent monograph of its own subject, they have necessarily much common ground in fundamental principles, and can scarcely be considered apart from one another by the student of nerve pathology. There is much similarity between the two works in their method of treating their subjects. Each is the product of untiring industry in an almost unlimited field; each is written with remarkable clearness and absence of pretension, and each is distinguished by a robust common sense, which leads their authors to avoid a prevailing vice of displaying erudition and making numerous quotations in settlement of some trivial point of detail.

Professor Charcot is as lucid in his arrangement and as interesting in his treatment of the subject as he always is, and if his speculations appear almost too precise and satisfactory in their application, it must be admitted that he supports them with such a powerful array of facts as to compel his readers' respect.

The French edition of Dr. Weir Mitchell's work is enriched by a valuable preface by Professor Vulpian, who pays a well-deserved tribute to the author's peculiar qualifications for dealing with his subject, the application he has devoted to it, and the distinguished success with which he has carried it out. Professor Vulpian reviews Dr. Mitchell's work at considerable length, and is in general agreement with him on the most important matters. The first point of difference is on Dr. Mitchell's conjecture that many maladies called "functional" are in reality due to congestion of nerve trunks. To this supposition he is led by the experimental fact that he has actually produced neuralgia on his own person by the action of cold on a nerve; and by previous experiment and dissection he had

<sup>1</sup> 1. *Leçons sur les Maladies du Système Nerveux fait à la Salpêtrière.* Par J. M. CHARCOT. Tome premier. Deuxième édition.

2. *De Lésions des Nerfs, et de leur conséquences.* Par le Docteur S. WEIR MITCHELL, Traduit et Annoté avec l'autorisation de l'auteur, par M. DASTÈRE, et précédé d'une préface, par M. VULPIAN.

3. *Lectures and Essays on the Science and Practice of Surgery.* By ROBERT McDONNELL, M.D., F.R.S., &c. Part II. 1875.

ascertained that the same means caused violent congestion of a nerve in animals. The conclusion seems fair enough that neuralgia can be caused by congestion of a nerve, and in the absence of all evidence to the contrary, that neuralgia in the human subject frequently is due to this cause. Vulpián, however, contends that no neuralgic nerve has ever been shown to be congested,—which is true certainly, for obvious reasons,—that if it had, the congestion might be the consequence instead of the cause of the neuralgia; and that he is “disposed to believe” that such maladies are due to “modifications of the nervous centres.” This modification seems to be “a high degree of excitability of the anatomical elements of the grey matter, referred to in another place” (p. 4).

Vulpián points out the omission by Dr. Weir Mitchell of all mention of neuritis descendens, but as Dr. Mitchell’s work is confined to injuries of nerves, and this form of neuritis is seen only as a consequence of lesions of the brain and cord, there can be no doubt that he omitted it advisedly and properly.

In considering the disorders of nutrition which follow lesions of the spinal cord, Professor Charcot deals first and at greatest length with those which concern the muscles. He classifies these muscular affections into two groups; in one the electric contractility and the general nutrition of the muscles remain unaltered for months or years after the lesion of the cord; and, in the other, atrophy and total loss of function follow very rapidly on the principal injury. The first group corresponds to those lesions of the cord in which the white substance only is affected, or in which if the disease involve the grey matter, it does not include the great multipolar cells of the anterior horns. To this class belong the various forms of sclerosis of the white columns, both that which follow lesions of the brain (descending wasting), and that which affects the cord primarily. In either case the disease may affect the white columns along the whole length of the cord without any direct affection of the nutrition of the muscles. Should, however, the disease spread to the anterior horns of the grey matter, muscular atrophy rapidly follows.

The second group corresponds to those lesions of the cord in which the great cells of the anterior horns or the centrifugal fibres directly proceeding from them, are involved in the disease. This group comprises two classes of lesions: one in which the motor cells are primarily diseased, the other in which they suffer together with the surrounding parts, in some more general affection, such as acute central myelitis or spinal apoplexy. It is, of course, only inferred that the motor cells are the structures originally affected, from which the disease spreads to the adjacent parts, but the evidence in favour of

this view is very strong. In some cases, indeed, no morbid appearance can be detected in the cord besides degeneration of these cells. In others the sclerotic change in the neuroglia, which is most commonly present, is confined to the area of a group of cells, increases in degree from the periphery to the centre of the patch, and is particularly intense around the individual cells. This is the pathological change which is found in cases of infantile paralysis, in which the early and complete loss of contractility and the rapid atrophy of the muscles are such striking features. In progressive muscular atrophy also, which offers essentially the same clinical features in a chronic form, this is the morbid change observed.

The cutaneous eruptions due to lesions of the spinal cord so closely resemble those due to lesions of the nerve trunks that it would appear there is no means of distinguishing them. In locomotor ataxy it is not unusual, when the attacks of darting pain attain extreme severity, for a cutaneous eruption to occur over the course of the nerves affected or in the districts which they supply. This eruption may simulate urticaria, or it may be papulous, pustular, or herpetic, and in severe cases gangrene of portions of the skin may occur. Seeing that these eruptions never occur unaccompanied by the peculiar pains of ataxy, it is natural to suppose that they are dependent on the same pathological condition,—sclerosis of the posterior columns,—and accordingly Professor Charcot relates cases in which such sclerosis was the sole morbid appearance found on autopsy. He is not content with this, however, and with his characteristic zest for extreme precision, he endeavours to show that the eruptions depend, together with the pains, on affection of the internal radicular columns, or those fibres of the posterior roots which run vertically for a short distance in the posterior white columns before they enter the grey matter. To explain the appearance of the eruption, Professor Charcot assumes the existence of a certain number of centrifugal nerves among these fibres, and makes the eruption depend on affection of them; but, as we shall show further on, this hypothesis is unnecessary.

There is no evidence to show that lesions of any other part of the cord will produce cutaneous eruptions.

Cases are on record of herpetic eruptions occurring simultaneously with hemiplegia, and affecting the paralysed side; but the dependence of such eruptions on the cerebral lesion, at first sight doubtful, becomes extremely problematical now that Professor Charcot has found in one case a separate lesion—plugging of one of the arteries of the cauda equina—quite sufficient of itself to account for the herpes.

One of the most striking of the nutritive defects which

follow lesions of the nervous centres is acute decubitus, that form of bed sore which is so extremely prone to follow extensive destructive disease of brain or cord. It differs in several points from the ordinary form of bed sore (chronic decubitus) which occurs in general exhausting diseases. Its onset is extremely rapid, and it may follow in a very few days, or even hours, the cerebral hæmorrhage or softening of the cord on which it depends; also the degree of pressure required to develop it is, or may be, very slight indeed. The mere resting of the ankles or the knees one against the other is often enough to induce it, and in rare cases it shows itself without there being the least sign of antecedent pressure.

The first sign of this ominous affection is the appearance on certain points of the skin, usually the buttocks, of one or more erythematous patches of variable extent and irregular outline. The skin is sometimes rose coloured, sometimes dark red or even violet, but in all cases the colour disappears under the pressure of the finger. In some rare cases there is a quasi-inflammatory swelling, which may be accompanied by acute pain if the seat of the mischief be not previously anæsthetic. By the day following, or the next but one, vesicles or bullæ have appeared about the centre of the erythematous patch. They contain a colourless or a reddish or brown liquid. At this stage the process may be arrested, and the bullæ or vesicles may disappear. But usually the raised epidermis is shed, leaving a bright red surface covered with bluish or violet spots, corresponding to a sanguineous infiltration of the skin of the subcutaneous tissue and even of the muscles. These livid patches rapidly extend and become confluent, and in a short time they are the seat of gangrene, at first superficial, but which soon extends deeply. The bed sore is then fully established. Subsequently reparative changes may take place, or in their stead the sloughing action may extend until the trochanters are laid bare and the deep vessels and nerves exposed. It may even progress so as to destroy the sacro-coccygeal ligament, lay open the sacral canal, expose the dura mater, or open the arachnoid cavity of the cord. When this state of affairs is reached, the inevitable result is ascending meningitis, which may be either simple or purulent, or a form described by Lisfranc and Bailarger as ichorous meningitis, in which the inflamed parts are bathed in a greyish puriform fœtid liquid.

Acute bed sore from cerebral cause may arise after any lesion of the brain, giving rise to apoplectiform seizures, whether injury, hæmorrhage, tumour, or other. It is confined always to the paralysed side, and has a very constant situation, being always near the centre of the buttock, a little above and to the



inner side. It has little reference to the amount of pressure to which the part may be subjected or to urinary irritation.

When the bed sore arises from spinal lesion it is almost invariably in the median line, spreading symmetrically to both sides, and in the sacral region; that is, just above and internal to the situation of the other. In those rare instances in which the spinal lesion is unilateral the bed sore is also unilateral, and in that case it is on the side opposite to the lesion, or on the same side as the loss of sensation. As a rule, spinal affections producing acute bed sore produce other rapid nutritive disturbances, such as atrophy of muscles. Usually the bed sore begins on the fourth or fifth day after the accident. There is some doubt as to whether a fracture high up or low down is the more favorable to the production of a bed sore. According to Ashurst, who has collected nearly 400 cases, the decubitus is more likely to occur the lower the lesion of the cord.

It is most remarkable that in hemiparaplegia—that is, in lesion of one lateral half of the spinal cord—there are developed on the same side as the spinal lesion loss of motion, followed rapidly by muscular atrophy with loss of electric contractility, and a peculiar form of joint disease; while on the opposite side to the lesion occurs, with the loss of sensation, acute decubitus, that is, severe nutritive change in the sensory organ. The vaso-motor paralysis occurs on the side opposite to the lesion—the side of the bed sore.

The lesions of the cord which may give rise to acute decubitus are of the most varied kind,—injuries, “acute myelitis,” whether spontaneous or of traumatic origin, and the acute exacerbations of chronic cord disease. In any case, as in acute decubitus from cerebral disease, the attack giving rise to it is always a sudden one.

Autopsy has shown no lesion absolutely peculiar to those cases of injury or disease of cord which are accompanied by acute bed sore; but Professor Charcot claims that, in the majority of cases, there is evidence that the injured or diseased point of the cord has been the seat of *inflammatory* mischief, and to this he attributes the bed sore. Clearly it is not due to pressure alone, for in hemiparaplegia it occurs only on one side, nor on irritation from urine and fæces, for the same reason.

With regard to the region of the cord, on the implication of which the bed sore depends, it appears, from the frequency with which the latter occurs in central hæmorrhage and acute central myelitis, that the grey matter plays a chief part in its production, and since we know that in infantile paralysis, in which acute bed sore is very rare, the anterior horns are very extensively and suddenly diseased, we may fairly exclude them,

and consider the occurrence of acute bed sore due chiefly to affection of the posterior grey horns. To these must be added the posterior white columns, since it has already been shown that lesions of these parts may determine cutaneous eruptions, and in some cases sloughing of the skin.

The disorders of nutrition consequent on lesions of the nervous centres not unfrequently have their seat in the joints, and arthritis from this cause is divided by Professor Charcot into two varieties. The first form comprises those cases of acute or subacute arthritis accompanied by swelling, redness, and often by more or less pain. Joint disease takes this form when it occurs after injuries to the cord. To this variety belongs also the arthritis that occurs in hemiplegics. The joint affection in these cases occurs on the paralysed side, and usually in the upper extremity. It occurs more often in softening than in cerebral hæmorrhage.

The arthritis usually comes on from a fortnight to a month after the apoplectic attack, that is about the same time as the permanent contraction of the muscles, and is accompanied not only by the outward signs of inflammation but by changes in the joints of distinctly inflammatory nature; that is to say, nuclear proliferation in the synovial membrane, with increase in the number and size of the capillaries, and in severe cases sero-fibrinous exudation into the joint cavity, mingled with leucocytes in various proportions. The cartilages and ligaments do not usually participate in the change, but the adjacent synovial sheaths are acutely inflamed. This description at once puts us in mind of acute rheumatism, and in fact both the anatomical appearances and the physical signs in these joint inflammations are strikingly like those found in rheumatic fever; so much so that Professor Charcot is at considerable pains to draw up materials for the differential diagnosis, and failing to find an essential difference, grounds his diagnosis on concomitant circumstances, such as the existence of paralysis in the limbs affected. But surely a resemblance so very close as this, existing in essential characters as well as in more superficial appearances, can scarcely exist without a similarity at least in the nature and cause of the disease. At any rate, here seems to be an important argument in favour of the hypothesis of the nervous origin of rheumatic fever.

The second form of joint disease dependent on lesion of a nerve centre is that which occurs in locomotor ataxy, a most peculiar and interesting form of disease, for a knowledge of which science is indebted to Professor Charcot. The arthritis usually appears at the same period of the disease as the muscular incoordination. The first appearance is a gradual swell-

ing of the whole limb, occurring usually without pain and without febrile reaction, and attaining often an "enormous" degree. After some days the general enlargement disappears, but in the neighbourhood of the joint the swelling remains, formed partly by effusion of serous fluid in the joint, partly by distension of the periarticular bursæ. One or two weeks later crepitation in the joint reveals the existence of bone disease; then the fluid is reabsorbed, leaving an extreme mobility of the joint; and in a remarkably short space of time the heads of the bones are eroded, and in great degree absorbed. This alteration, combined with the looseness of the ligaments and the atrophy of the muscles which often follows, allows of "spontaneous" luxations of the joints.

Professor Charcot believes that he has localised, in the anterior grey columns of the cord, the change which gives rise to these joint affections.

The interference with normal nutrition caused by injuries to the central nervous system is not confined to the skin and the locomotor organs. The viscera also are liable to trophic changes from this cause, though as a rule the alterations are neither so frequent, so extensive, nor so serious, as those already considered.

The changes in internal viscera dependent on lesions of the brain, whether of the ganglia at the base, the pons, or medulla, are confined to simple neuroparalytic hyperæmia and ecchymosis, appearing in the pleura, the endocardium and the mucous membrane of the stomach. Whether the pneumonia which is frequent in the course of certain brain affections is also directly dependent on the nervous lesion is very doubtful.

Lesions of the cord also are occasionally followed by internal ecchymoses, especially in the supra-renal capsules; but the most important consequence of cord injury which falls under this head is the inflammation of the urinary passages, which is so frequently the immediate cause of death. Professor Charcot draws attention to the extreme rapidity with which this affection follows the injury to the cord, and argues from this that the inflammation is directly due to disturbance of nerve influence ("irritation"), rather than to the local cause of decomposing urine, whether from spontaneous fermentation or from the introduction of organisms by catheterisation. He does not, however, advance any new fact in favour of his own view, and the general opinion of the day seems more in favour of the latter hypothesis.

The most important question arising out of the works under review is, What is the causation of these profound disturbances of nutrition which follow injuries to nerve-trunks and centres

and, in particular, What evidence do they afford as to the existence of trophic nerves? The existence of nerves having a special and peculiar influence over the nutrition of the tissues has been a vexed question for many years, and it is satisfactory to know that although the evidence is still defective, it has so accumulated of late years as to afford a reply, not yet certainly accurate, but probably closely approximating to the truth.

In seeking an explanation of these nutritive changes, it is natural to turn first to the great regulator of the activity of nutrition—the vaso-motor system, and to endeavour to account for them by the lesion which the vaso-motor suffer in common with the other fibres of the injured nerve. It is clear, however, that any affection whatever of the vaso-motor nerves of a part can have but one of two effects: it either increases or diminishes the quantity of blood flowing through the part, and can produce just those effects which are due to an increased or diminished blood-supply, and no more. Every item of evidence that can be adduced helps to show the impossibility of accounting for the trophic changes here considered by any affection of the vaso-motor nerves, and authorities are singularly unanimous in agreeing to this opinion. Neuro-paralytic hyperæmia will exist in an animal for many months after section of a nerve, without the least sign of such trophic lesions being evinced; and the removal of the upper cervical ganglion will even prevent the panophthalmitis and disorganisation of the eyeball which usually follow section of the fifth nerve. In man also the vaso-motor of a part may be permanently paralysed without the occurrence of any of the severe changes already described. Again, the opposite condition, contraction of the vessels of a part, either from wound of a distant part, as in some of Dr. Weir Mitchell's observations on men, or from direct irritation of the vaso-motor nerves, as in O. Weber's experiments, may exist for a long time without producing any such changes as those here considered. The trophic defects that are observed take place also under the most variable conditions of blood supply, and there is no sort of correspondence between the kind or degree of the trophic lesion and the paralysis or spasm of the blood-vessels of the part affected. (There is one exception to this, and that is the well-known fact that parts whose vaso-motor is paralysed are apt to inflame if the animal fall much out of health.) Similarly, vaso-dilator nerves cannot but be quite inoperative in causing, and can exercise a very subsidiary influence only in accelerating, nutritive changes.

Setting aside, then, vaso-motor lesions as affording no adequate explanation, we have next to ask how far the complete inactivity to which a muscle is reduced by loss of innervation

can explain the atrophy which it undergoes, and how far the ulcers, &c., on skin anæsthetic from loss of innervation are due to the ordinary influence of external agents acting on a part which is unable to feel an injury, and unable to communicate to the organism the necessity of removing it from hurtful influences.

It would seem that the extreme rapidity of the atrophy and its profound extent, both far greater than in the atrophy which follows even the most complete inactivity from other causes, are sufficient alone to negative this hypothesis; but Professor Vulpian adduces another argument which, though rather indirect in its bearing, must be allowed some weight. After recapitulating the Wallerian experiment of dividing the posterior roots of the spinal nerves, some on the distal and some on the proximal side of the ganglion, with the result of producing atrophy of the peripheral ends in the first and of the central ends in the second case, Professor Vulpian argues that, as in each case the carrying function of the posterior roots was wholly destroyed, atrophy of nerve-fibres after division cannot be ascribed to functional inactivity, and if not of nerves, clearly not of muscles, which are less directly involved. Although we agree with Professor Vulpian's conclusions, we cannot quite accept his reasoning; for, irrespective of the assumption that what is true of nerve-fibre is also true of muscle, few pathologists will admit that the carrying function of the posterior roots is destroyed so long as their central extremity is in communication with the grey matter of the ganglion. The prevailing opinion seems to be that where there is grey matter there is a reception, retention, and redistribution of force; and so long as this is the case, the nerve-fibres attached to them cannot be said to have lost their function. Still, setting aside this reason, it must be maintained that the rapidity and extent of muscular atrophy of nervous origin are so much greater than those from other causes that we are obliged to consider it due to some special agency and not to mere inactivity. The same is true of other trophic lesions. The one fact that in hemiplegia and hemiparaplegia the bed sore appears first, and is always most severe on, and often entirely limited to, one side, is enough to prove incontestably that it is not due simply to the irritation of decomposing excretions, and the inability of the patient to alter his position. The nature of the lesions, too, is peculiar. The indolent punched-out ulcers occurring on fingers which have lost their nervous supply are never seen to result from mere pressure on a part whose nerves are not paralysed, however severe and long-continued the pressure may be. Pressure sores will indeed result, but apart from the longer time required to

produce them, their clinical characters are readily distinguished from those on paralysed limbs.

Since, then, the vaso-motor effects, and what we may call the necessary mechanical effects of loss of innervation are not sufficient to account for the peculiar disturbances of nutrition which follow this loss, we are compelled to suppose that the nutritive defect is the direct consequence of the injury to the nerve. This nerve injury may be conceived to act on the part affected in one of two ways,—either by the loss of some influence which the nerve transmitted to the periphery under normal conditions, or in the transmission of some new and hurtful influence from the injured portion of the nerve to the part which becomes the seat of the defect of nutrition. The latter view is advocated by Professor Charcot, the former by Professor Vulpian, and very guardedly by Dr. Weir Mitchell. Professor Charcot maintains, and brings forward a considerable body of evidence from both human and comparative pathology to show that trophic defects follow those nervous lesions only which are, or may be, accompanied by neuritis, and that it is the “irritation” propagated from the seat of injury to the periphery, which determines the atrophy of muscle and the depraved nutrition of skin. It must be confessed that there are great difficulties in the way of accepting this solution. The evidence of neuritis, or what Professor Charcot calls an “irritative lesion,” is far from complete; and indeed in the majority of cases it can be shown only that the conditions for such a neuritis are present, not that it exists. Professor Charcot admits that every neuritis is not necessarily followed by trophic troubles at the periphery. Besides, as Professor Vulpian argues with much force, how can we admit, even supposing that there is inflammation of the injured nerve, that any influence, “irritative” or other, can be transmitted by nerve-fibres, which lose in a few days after section both their normal structure and their physiological properties? In view of this, the objection that the lesion of the periphery is not necessarily “irritative,” *i. e.* inflammatory, but is often purely atrophic, becomes almost unnecessary.

By a process of elimination, therefore, we are compelled to adopt provisionally the hypothesis that the peripheral nutritive defect is due to the loss of some influence which, under normal circumstances, is conveyed by the nerve injured. In other words, that there are nerves which exert a special influence on the nutrition of parts to which they are distributed, an influence which entitles them to the name of trophic nerves.

The evidence given above is not the only evidence in favour of this view. Recent microscopical researches have discovered

the terminations of nerve-fibres in the corpuscles of the cornea, in connective tissue cells, in muscular fibres, and in the secreting cells of glands. Everywhere they are found having the closest relation to, or actually terminating in, the tissue elements; and even if we had no evidence on the point, it would be incredible that currents passing along these nerves should not exercise a profound influence over the processes taking place in the elements to which they are distributed. But experimental evidence of the most conclusive nature is forthcoming to show that nerves do exercise a very powerful direct influence on the nutrition of the tissues. The well-known experiments of Ludwig on the submaxillary gland show that irritation of a nerve which supplies a gland can cause great increase of the secretion of the gland under the most various conditions of blood-supply, and even when the latter is wholly arrested. Since secretion is only a modification of ordinary nutrition, it follows that nerves do in some cases exert a direct influence on the nutrition of anatomical elements, wholly independent of all variations in the blood-supply. Again, seeing that it is utterly opposed to scientific consistency, and to what we know of the uniformity of plan of the whole body, to suppose that what exists in one part has no parallel in a part but slightly different in other respects, it seems a fair inference that nerves possess a direct trophic influence over all tissues into whose elements they have been traced.

The existence of trophic nerves is, therefore, highly probable, but there is still an uncertainty as to whether the trophic function is exercised by a special set of nerves devoted solely to this purpose, or whether it is carried by the ordinary nerves of sensation and motion contemporaneously with their functions. This question seems to be tending more and more to be settled in favour of the latter view. Professor Vulpian, although he gives the great weight of his authority in favour of the existence of a trophic influence of nerves on tissues, cannot admit the existence of special trophic nerves. Dr. Weir Mitchell can neither allow the probability of their existence nor see the necessity for them, and Professor Charcot takes essentially the same view. All alike scout the idea of sensory nerves transmitting influences in a centripetal direction only. It certainly would seem a clumsy expedient, and very foreign to the general simplicity of the organism, for there to be two sets of nerves going to the same organ carrying influences so closely interdependent as those relating to physiological action and nutrition.

Attention has already been drawn to the circumstance that in lesion of one lateral half of the cord there occur, on the same side as the muscular paralysis, muscular atrophy and disorgan-

ising joint disease. That is to say, the same lesion which abrogates the locomotor function of the part produces serious trophic changes in the locomotor organs of that part. On the opposite side, loss of sensation is attended by acute bedsores, that is, by trophic change in the sensory organ. Hence it appears that the nerves carrying trophic influence have an identical course in the cord as well as in nerve-trunks, with those which carry motor and sensory influences. It does not, on that account, necessarily follow that the fibres carrying the trophic influence are the same as those carrying motor and sensory impressions, and whether they be so or no will have to be settled with the general question of whether one set of nerves carries all impressions, or whether there is a separate set of nerves for each kind of centripetal and centrifugal current. For if, as Dr. Brown-Séquard and others maintain, impressions so nearly alike as those of touch and tickling have separate nerves for their transmission, a separate set must also be supposed for the trophic influence.

Notwithstanding the strong evidence in favour of a plurality of sets of nerves corresponding to the plurality of influences transmitted, which has been adduced by many eminent men, it must be admitted that this question is far from being settled. Varieties of sensation shade off into one another by such infinite fineness of gradation that it is impossible to mark the limits between them; and it seems inconsistent to assign a separate set of nerves for each end of the scale, to tickling and to touch for example, unless each intermediate degree has also a set; and even without going so far as this, the number of different sensations is so great as to necessitate, on this hypothesis, an almost infinite number of sets of nerves—an assumption which is not warranted by the facts of anatomy. Again, the fact that the cross union of nerves after division has been effected, so as partially to restore function, shows that in structure the nerves carrying very different influences are not essentially different. Another hypothesis has been put forward by Dr. McDonnell which deserves consideration both for its simplicity and for the harmony which it seeks to establish between the phenomena of the transmission of nerve-currents along nerves, and that of other forces in other media. The supposition is “simply an application of the theory of wave propagation to the passage of various sensations along nerve-conductors.” “I conceive,” says Dr. McDonnell, “that the various peripheral expansions of sensitive nerves take up undulations or vibrations and convert them into waves capable of being propagated along nervous tissue (neurility, as it has been well named by Lewes). Thus the same nerve-tubule may be able to transmit along it



vibrations differing in character, and hence giving rise to different sensations ; and consequently the same nerve-tubule may, in its normal condition, transmit the wave which produces the idea of simple contact, or that which produces the idea of heat ; or again the same nerve-tubules in the optic nerve which propagate the undulations of red, may also propagate in normal vision, those which excite the idea of yellow or blue, and so for the other senses." According to this hypothesis impressions differing both quantitatively and qualitatively can be transmitted along the same nerves at the same time. But, in order to substantiate the theory, Dr. McDonnell must be prepared to show that, in its transmission along the nerves, the nerve-current shows the same phenomena of combination and interference of waves as obtains in other cases of undulatory transmission of force.

To recapitulate: the present state of knowledge with regard to trophic nerves seems to be: 1. That there are nerves which transmit currents having a direct influence on the nutrition of tissues. 2. That in health these currents, like those which give "tone" to muscular tissue, are constantly passing. 3. That the interruption of these currents by damage either to nerve or nerve-centre causes disorder of nutrition in the tissue. 4. That whether the nerves which transmit these currents are specially devoted to this purpose, or are the ordinary nerves of sensation and motion, there is not yet sufficient evidence to determine.

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### IX.—Biological Foreshadowings.

EVERY one who can work tolerably well with a microscope, and who reads the histological literature of the day regularly, has his hours of quiet thought often interrupted by reverie ; that is to say, if he has any philosophy in him, and is not content to move on in the prescribed groove. One sees things in different creatures which evidently have some relation to a continuous idea in nature ; and as the intermediate beings become known, the persistence of certain kinds of structures endowed with common attributes through great series of species and genera becomes established as a fact in the mind. The connection between increased differentiation or complexity of structure and higher subjective attributes, and its foreshadowing by the simpler nature of the lowest forms of life, strike every thoughtful observer. Such things tempt those who occasionally indulge in

biological castle-building to give free scope to the scientific use of the imagination. Doubtless much of such reveries is of no great value; but, nevertheless, wandering thoughts are pleasant relaxations, and their communication to other men is often instructive. The constant devotion to induction is, doubtless, highly proper; but, nevertheless, any little flirtations with deduction are the agreeable wickednesses of science, and they refresh the mind and stimulate the observer. The following essay must be considered as suggestive rather than didactic, for it relates to many ideas which, whilst they refer to facts, are incapable of demonstrative proof, and it has been written with the wish to interest microscopists in some of the thoughts—many, doubtless, evanescent enough—of the day.

The changes of shape of an *Amœba*, and the contraction and extension of its structureless substance, are so frequently and readily to be seen that the feeling of wonder they excite in the first instance soon passes away. But after these simple phenomena have become very familiar, the first view of a human white blood-corpuscle clinging to the tissue close to it, slightly projecting one portion of its surface, retracting another, and the whole, as it were, streaming onwards irrespectively of any external influences, not only recalls the former impression of surprise, but adds greatly to the feeling of intense interest and wonder. The movement is like that of the minute organism; it is as mysterious, from the absence of fibre and adapted tissue, so the mind connects the microcosm with the macrocosm in a physiological phantasy. It may happen, during carefully conducted microscopical investigations, that the eye, acquainted with these strange movements, may detect them in the corpuscles of the lymph of mammalia, and may become assured that cells with elongate processes, and which belong to connective tissue, move and penetrate the homogeneous walls of the minutest capillaries and lymphatics. Or, under favorable circumstances, a faint spontaneous movement may be observed in the anterior epithelium cells of the cornea and in some cells in the corneal tissue itself. Again, the pus cell gives evidence of active change of shape, and the large granule spheres met with in exudations within serous membranes have processes capable of the peculiar movement, and which attach to themselves granules, and thus aggregate granule on granule. The multiplicity of examples demands some more satisfactory conclusion than a fanciful generalisation, and the mind, in seeking for light, necessarily stimulates investigation in the histologies of the great groups of beings which lie between the vertebrate and the simplest forms of life, so as to endeavour to comprehend this *βιοκίνησις*.

The result of years of histological labour has been to prove that all through the animal kingdom, and also in the groups of organisms whose animal or vegetable nature is uncertain, these life-movements, both slow and quick, are more or less present.

Take the freshwater Hydra, for instance, and examine its internal structure, employing different chemical reagents, such as very dilute perosmic acid, dilute mineral acid and a solution of chromic acid, after having attempted to prepare for the microscope without them. The endoderm will be found, in the unaltered state, to resemble masses of amœboid appearance with contractile spaces or vacuoles, and the mass moves whilst the vacuoles increase or diminish in size. The reagent gives the appearance of a cell-wall which limits the masses and forms an endothelium. In structure, in movement, and in physiological attribute each of these masses has the characters of an Amœba. The greatest amount of possible movement is in and about the region of the vacuole. Now, in the same animal there are certain grotesquely-shaped bodies with nuclei and with filiform and even branched prolongations, and their structure is minutely granular and also homogeneous. They have the power of altering their shape, of expanding and contracting, and, moreover, of communicating their movement to those others which are touched by their processes.

Again, in the Actiniæ, if a tentacle be nipped off and a transverse section be made, and the necessary thin glass be placed on so as not to crush too much the apparently confused mass of cells, some remarkable alterations in their forms may be observed.

Some of these cells are usually globular in shape, and have a thin cell-wall and glairy contents, and they may retain this character for a considerable time. But sooner or later it generally happens that as the observer is, perhaps, drawing them, their outlines alter, and several or all move slowly, and produce a general revolution in the form of the examined mass. As in the Hydra, there is general as well as individual cell-movement, and its resemblance to that of the Amœba is remarkable.

These are two instances in which the most elementary structures plainly resemble the histology and physiology of the lowest forms.

Between these Cœlenterata and the Amœbæ there is a vast hiatus, filled up to a certain extent by the Spongida and Rhizopoda, and, as might be anticipated, the amœboid movements are to be distinguished in them. In the common freshwater Spongilla the superficies is composed of a thin layer of sarcode with small, pore-like openings in it, which lead to a cavity between

it and the bulk of the sponge; and the sarcode is composed of nucleated cells placed side by side. They can alter their shape, project pseudopodia, retract them, and can include nutrient particles when in the mass; and the resemblance to the amœboid condition is all the greater from the power which these cells have, when separated, of continuing their remarkable evidence of vitality. Moreover, in the American Spongillæ a structureless protoplasm lines certain chambers, which correspond to the canals of the European form, and it has a power of contraction and dilatation sufficient to determine, according to Clark, an expulsion and a corresponding inrush of water.

Globigerina, as a type of the division of the Rhizopoda, instances the streaming movement of protoplasm with granules within it to perfection, and the phenomena of pseudopodial emission and retraction are exhibited as necessary physiological movements. In the spinose kind, the foramina on the outside of the shell open out into hexagonal spaces, from the angles of which long and hair-like spines stand forth. The sarcode emitted from the chambers of the foraminifer comes through the foramina being also continuous with an exceedingly delicate layer which covers the outside of the shell. The pseudopodium streams out, its particles move over each other, like those of running gum; it covers everything, clasps the spine, envelopes it, and extends beyond. On irritation, or in relation to what is hard enough to define as "will," this onward streaming is reversed, and nutrient particles are enveloped and escorted back within the animal.

In the freshwater Gromia, which is occasionally found on the pond-plant *Ceratophyllum*, the approach to Amœba is stronger. It has, however, a shell or carapace, globular or ovoid in shape, and brownish in colour. Masses of sticky, streaming granule-pseudopodia are emitted to a considerable distance, and then the sarcode runs together so as to form a close reticulation, or else a tenacious mass. It is capable of including nutrient particles and very small animals; it has vacuoles which expand and contract, and the whole is in constant motion, either as a mass or in part contracting, dilating, elongating, thinning, diminishing, and always streaming. It is tolerably certain that portions of these rhizopodal sarcodes when detached retain all the peculiarities of the mass, and they certainly do so in Amœba. Labyrinthula may also be instanced: it is microscopic, and consists of thin reticulate, colourless filaments. It has no shell, and the whole mass can change its shape almost indefinitely. The protoplasm can include, assimilate, separate and unite, and the slow streaming circulation of its molecules may be proved by the movement of curious fusiform bodies; and it is also evident

that the filaments aggregate in masses now and then, or else occasionally form globular masses.

Again, in ascending the scale above the Cœlenterata, the special slow movements of cells amœboid in nature have been seen in almost every group whose species have been examined carefully. The migrating cells in the worms described by Ray Lankester, and the changes of shape in the pigment cells of Cephalopoda are identical in their phenomena with the movements of the pigment cells of the frog which Savioti has observed to pass into the capillaries, and to be carried along in the stream of blood, and also with the migration movement of mammalian blood corpuscles and pus cells.

Thus, all through the animal kingdom, and, as is well known, through a vast series of plants and indefinitely classified organisms, there are examples of slow-streaming, contractile, extending, thinning, and thickening movement within separate cells, or in tissues cellular or not. Its significance is not diminished by its being observed in the impregnated ovum of mammalia, amphibia, birds, and fishes. The ovum undergoes certain automatic changes in form in them even before the cleavage sets in. Thus the partly deposited ova of the toad have several facets, but they subsequently become spherical; and Stricker has shown that the yolk-cells of the trout undergo amœboid movements, such as contraction and dilatation. Hence it may be assumed that the simplest phenomena of what we call life, or the slow molecular movement of structureless organic matter, is witnessed throughout all living forms, and was so in the past.

Amœba and its structural resemblances in low, simple, and compound organisms, with its wall-less protoplasm, and also Protamœba, which has no vacuole even, absorb organic matter and assimilate it. Or, in other words, the molecules of extraneous things consisting of C, H, O, N, and possibly without the nitrogen, are rearranged in its substance, both chemically and physically. Clearly all this opens the understanding to the comprehension of many phenomena within the vertebrate animal. Formerly men's eyes were dimmed by groping along in the dogmatic darkness of authority which taught the universality of basement membranes and cell-walls in relation to absorbent and eliminating functions, which inculcated the necessity of the presence of special structures for the production of movement, and which sought to environ every evidence of life with a nervous system. Now the recognition of the repetition throughout the animal scale of the amœboid structure and attendant powers, enables the marvellous gifts of undifferentiated protoplasm to be acknowledged.

In continuing the subject it must be remembered that the

animals included in the Amœbina usually have vacuoles, and divide spontaneously. The Protamœba has no vacuole, however, and therefore has nothing in the form of rhythmical movement, its crawling and curious change of form being, however, very automatic. The next step in the scale finds Amœba with a contracting vesicle, that is to say, the molecules of a portion of the protoplasm have the power of moving away radially from a common centre and of returning. The result is to produce a spot through which transmitted light passes more easily than through the rest of the mass. A fluid, probably water, occupies the space for the time being, and it is not restricted to one spot. These vesicles and vacuoles are found in the amœba cells of Hydra, in sponge cells, and throughout the vast tribes of Infusoria, but they cease to be recognised high up in the animal scale. They are the first stage of an important local differentiation, for they evidently relate to the development of very contractile local spots within the unicellular bodies of the lowest animals. The contractile vesicles so common in the Infusoria offer examples of the simplest kinds of rhythmical movements, and of movements more decidedly active and quicker than that called amœboid; they open and close and some are connected with the outside by a tube-like prolongations, and they move in systole and diastole with varying rapidity. In some infusorial species the contractile vesicles have radial prolongations, but they, and the cavities themselves, disappear on active contraction taking place. Moreover, an internal swarming movement of the protoplasm, which is not seen on the outside of the animal, carries vacuoles and contractile vesicles here and there even in large Amœbæ. All this movement is repeated over and over again in the Infusoria, and it occurs with all the attendant phenomena of absorption and special movement in the amœba-like spores and filaments of the myxogastres whose complex life-cycle leads to a plant at last. It may then be considered automatic and altogether beyond anything approaching to the result of consciousness.

The next advance in the ability to produce more rapid movement of their molecules is seen in a thinning of the edge of the many protoplasmic bodies, and its becoming subject to a wavy motion, assisting therefore in the locomotion of the animal, or, should it be stationary, producing water currents.

Thus in one of the Trichodina with a discoidal body, and which chooses the bodies of *Hydra vulgaris* and *H. viridis* as its roaming ground, in spite of their terrible artillery of nematocysts, the lower part has a thinning off in the form of a ring-shaped membrane. This acts both as a sucker and as a means of locomotion. In the blood of frogs and fishes an

actively moving film of protoplasm may be found; it is the *Trypanosoma* of Siebold and the *Undulina* of R. Lankester. It is thin, and is thinned off to the edges, which have a rhythmical power of contraction and dilatation, and moreover one part contracts before the next, and thus a vibratile motion is given. This membranous appearance may be traced in the higher animals on examining immature cells, which eventually have active movement; for instance, a series of cells of the lung of *Ostrea* often exhibit this wavy movement in a thin film, which is on their free surface, but it soon splits up into separate rods, each of which then constitutes a cilium. Again, these undulatory membranes occur upon the spermatozoa of salamanders and tritons, and in the water-vessels of some Annelida, such as the *Turbellaria*. These membranous expansions, thus carried on in the developing scale, are the foreshadowings of ciliary cells, or of protoplasmic masses with vibratile processes.

They are the foreshadowings also of rapid rhythmical movement in more highly differentiated protoplasm, or, in other words, the structures of higher animals. The simplest rhythmical movements in the organic kingdom which have any amount of frequency and velocity are those which are characteristic of the active stage of the complex life-cycle of the protista—of those beings whose position in the scale of classification cannot be determined to be always animal or always vegetable. The *Astasiæ* are an important group of the *Flagellata*, and some of them are the zoospores of unicellular algæ, such as the red snow and other protococcoid beings. They have during their motile stage one or more prolongations of their sarcode, in the form of long, bristle-like processes; these are the flagella, and they protrude through the delicate cell coating, or in some forms are continuous with a contractile outer integument homogeneous in its nature. The *Astasiæ* with two of these flagella at one end, or with only one process, are common objects, and the rapid vibrating movement of the bristles in regular rhythm is readily seen. Equally common are the so-called zoospores with flagella, of *Gonium* and *Volvox*, and in their movements there is often exquisite regularity and successive contraction. In such instances the successive contractions and relaxations of the tension of the flagellæ of a number of grouped cells give definite motions to the mass, and the whole moves towards the light. An exaggerated flagella worthy of the name of flagellum is noticed on *Noctiluca*. It is a rhizopod, about the size of a pin's head, and it has a stout tentacle-like process, which is curved and striated, by which it propels itself through the water. This flagellum rises from a little depression in the body close to a canal, which leads into the granular sarcode of the body, there

being no definite stomach or intestinal canal, the food being absorbed, digested, and assimilated in the glairy mass. The motion of the flagellum is not produced by any differentiated structures, but simply by the active rhythmical, and therefore intermittent, action of the protoplasm.

Then the Oxytrichina, a family of infusoria, have in many species numerous flagellum-like cirri or setæ; these may be at rest and the animal may move with ordinary cilia. But every now and then it moves them with great rapidity, and bounds through the water so that the eye can hardly follow the rapid saltatory movement. They are the crawling Infusoria, and have hooks to cling on by besides those powerful setæ. In the Halterina the spherical or top-shaped body is surrounded by long and very delicate flagella-like cilia, which like those of the Oxytrichina act by suddenly contracting upon the surface on which the animal rests, and jerks it forwards. Now, all these processes are but undifferentiated protoplasm; on the one hand they resemble the pseudopodia of the Rhizopoda, and on the other, they may gradate into the common cilia. Thus, the processes of the Acinetina act as catchers of prey, and absorb the nutrient parts; and the stout setæ of the Oxytrichina have been seen to split longitudinally and to become cilia.

The cilia so common in almost every great group of animals from the spongida to man are, of course, the most prominent examples of protoplasmic and rhythmical movement. Whether examined in an infusory animalcule, in a Rotifer, in the gill of a Mollusc, or in the mucous canals of vertebrata, they may be seen under two and rarely under a third condition. They frequently are simple, hair-like prolongations of a wall-less mass of protoplasm in which there may be a nucleus, a vacuole, or a contractile vesicle, and there may be one or more of them to the mass. Or they may be prolongations of a cell-wall which encloses protoplasm and a nucleus, and in both of these instances the movement may be in one direction from the point of rest, and of course there is a return to this point. It occurs in some instances, however, that the movement is more extensive, and the cilium does not simply bend from a straight direction and return by its elasticity, but a force wags it so as to make the top describe a large part of a semicircle in the same plane throughout. Contractile energy exists at the base of the processes, and it acts rhythmically.

But the third condition of ciliary motion is evidently under the control of the animal, as it can be readily seen in the "wheels" of the rotifera. In this kind of ciliary movement there is then not confusion but serial effect, even along long lines of cells, and



the alternate action and cessation of contraction which produce the rhythm, continuous as long as the cells are nourished and properly aerated.

The passage from a long cilium which is evidently moved not continuously but occasionally and in relation to the special requirements of the infusorial being, to a long thin structureless band of protoplasm attached at both extremities—to a contractile muscular fibre like that of *Vorticella*—is made through the setæ of such organisms as the species of *Campylopus*. And as the jumping legs of that genus act apparently in relation to the will of the creature, so the contraction of the fibre of *Vorticella* is evidently connected with the distant crown of cilia and their movements in collecting food. It is hard to use the words will and volition, but nevertheless it is evident that as the protoplasm of animals ceases to be simply such and becomes differentiated, another power, as mysterious as movement without definite structural adaptation, comes into play—the ability to move in relation to surrounding wants, and this is not entirely automatic. Thus, as has already been noticed, the simple cells of *Hydra* with a differentiated fibre at their end (with no other structure, however, than that of a granular sarcode) are half muscle, half nerve in their office; they contract and extend their contraction to their fellows. In the *Vorticella* the contractile fibre-like thread of protoplasm is attached to some object or other below, and it passes into a mass of granular sarcode at the base of the bell-shaped body. Its contractions resemble those of the *Hydra* and also of the simple muscular fibrils of the tentacles of *Actinia*, and even of the equally simple fibres seen in the intestinal canal of some *Insecta*. The connection with the granular mass is evidently important, and the movement does not commence there but at the lower end, although the order to contract does. Clearly these are foreshadowings of the more perfect involuntary muscular fibres of higher animals, whose nervous supply is often so scanty but whose rhythmical movements culminate in peristaltic and in systolic and diastolic movements.

Before passing on to the consideration of rhythmical movement in fibres and cells in animals higher than those already noticed, it is necessary to remark that as the curiously shaped cells of the *Hydra* are musculo-nervose and can contract and stimulate their neighbours to do the same and more or less in rhythm, so something of the same kind of structure exists in the *Actinia* and determines active contraction both of the automatic and volitional kind. The tentacles have in their midst, but not in direct organic contact with the delicate motile fibres, cells with irregular projections resembling some

multipolar cells of the vertebrate nervous centres. These cells are subject to amœboid movement, and so are the motile fibres; and when the cells are, so far as is known, at rest, the fibres can contract on the application of direct stimulation, or from the physiological necessity of so doing owing to general requirements, such, for instance, as a diminished quantity of water and included air, or the accession of nourishment from the recent digestion of matters within the visceral cavities. Under the first of these circumstances a tonic and continuous contraction of the motile fibres occurs, and retraction of the tentacle supervenes; and under the second condition the same kind of contraction is accompanied by the circulation of protoplasm in the tentacles, and its formation into matters like those which have gradually diminished in bulk on account of wear and tear. This tonic contraction which closes the *Actinia* is a process of shortening of the fibres—of action and not of relaxation, and it appears to have a definite connection with the cessation of amœboid movement. Are amœboid movements in the nerve-cells or their foreshadowings, and which are communicable to others of the same kind, interchangeable with the muscular force, or rather with the something that causes the contraction of the fibres? When the amœboid movement does not occur the other does, and moreover rhythm is often to be noticed. Considering, then, the amœboid movement to be the first stage of the nervous force and to present phenomena which foreshadow what is called volitional movement, it is not surprising that the perfect will should be represented in the lowest animals by merely rhythmical and also by automatic movements—movements which under abnormal conditions of the nervous system in higher animals sometimes replace those of the will. The mass of the highly irritable muscular fibres—mere rudiments of involuntary muscular fibres—which form the layers of the *Actinia*, are crossed by irregular-shaped streaks of granular sarcodæ which here and there thin out into fibres with enlargements. These are the primitive nervous elements of the body and their resemblance to the adynamic and the conducting elements of the nervous system of higher animals is remarkable. They are rare, and it would appear that the crossing by one nerve of any number of muscular fibres is sufficient. It is very probable that the irregular sarcodic mass is competent to change its shape, and if this can be proved it certainly will be a most important link in the theory which I venture to offer, that the potentiality for amœboid movement is transferable to non-amœboid structures as nervous energy, in the first instance, as the force which produces rhythm, automatic movement, and volition.

It is interesting to notice in the lowest animals, when the rudiments of a nervous system are observable, that two simple structures are necessary for its operation—a mass of protoplasm and a prolongation, or a mass of protoplasm with a cell wall, part of which is extended, or a granular mass of sarcode gradating into thread-like filaments with occasional swellings. All these foreshadow a mass of granular nerve substance—bioplasm or differentiated protoplasm, part of which elongates and forms a nerve axis-cylinder. Just as the slightest differentiation of protoplasm in the lowest animals is accompanied by some further development of potentialities, so with increasing differentiation up in the animal scale comes increased nerve force, so that the pale of simple rhythmical and automatic movement is soon passed.

What amount of structural differentiation is required for the ability to “will” is, of course, a most interesting question. It is one which is, however, complicated by the difficulties of distinguishing “will” from automatic action, and of knowing where automatism ends and volition commences. Moreover, it is as yet impossible to say with perfect truth where nervous matter begins in many of the lowest animals which certainly give some evidences of volition. The possession of organs of special sense would seem to give a hint to the necessary existence of ability to exert a certain amount of volition, but there is a difficulty even in deciding upon the nature of the rudimentary structures which physiologists and histologists recognise as sensory organs. If this last difficulty be carefully considered and explained in some animals, it will illustrate the general nature of the complexities which surround the subject. Take the common sea anemone, *Actinia mesembryanthemum*, and notice its habits. Its usual position is on rock within the tidal zones; and not only is it sometimes uncovered by water during low tides, but it often moves up out of the reach of the sea, and spends a certain time closed up, and yet strongly adherent by its base. If they are kept in aquaria their migration soon becomes a familiar subject for consideration. The animal, moreover, chooses a stone or hard object which is suitable for its base to clasp, and avoids others; and if it cannot find one, it often casts itself loose and floats. It requires well aerated, pure sea-water and a moderate amount of simple food; and during its period of occlusion, as has been already noticed, secondary assimilation appears to progress rapidly, and the tentacular structures undergo improvement in their nutrition.

In these Actiniæ there is a trace of special sense, a structure which permits of light entering into the body further in some parts than in others, where it may stimulate some peculiar

bodies, very irregular in shape, and which, from analogy with corresponding cells in the tentacles, are probably capable of movement. This structure is a modification of the epiderm, and foreshadows the more perfect eyes of higher animals, organs which are equally modifications of dermal structures. The histological elements of the skin of the *Actinia* are, 1, globular cells, with a glairy mass within, the cells being very large when mature, and of all sizes, down to that of a point during development; they afford the slimy exudation. 2. Nematocysts, or thread-cells. These are oval or elongate cells, placed often side by side, their tips appearing flush with the surface like so many cones, and each contains a coiled thread of offence. They are much larger in some places than in others, and sometimes are long and cylindrical in shape, the ends being conical and the cell-wall very stout. 3. Granules and granule-cells. These are the colouring matter of the *Actinia*, and they crowd in between the other histological elements. Now, in the *Actinia mesembryanthemum* a row of very pretty turquoise-coloured bead-like structures is situated at the base of the tentacles on the outside of the body, and just within a fold of skin which borders them. The bead-like structures are shiny, prominent, and gradually merge around into the common integument. On examining them it will be found that they present the histology of a visual organ, thanks to modification in the cells of the skin and their special arrangement. On the outside of the chromatophores, as these bodies are called (see "On Nervous System of Actinia," 'Proc. Royal Society,' vol. xxii, No. 151), there is a layer of small elongate cylinders, exactly resembling the smaller nematocysts, but they have no thread within, only a refractive and pure protoplasm. This layer is composed of thin, elongate cells, placed side by side in rows, and having one of their conical ends free and the other embedded in the next layer of tissue. These elongate cells or bacilli are separated from each other by extremely delicate layers of opaque granules and granular protoplasm. Their internal ends are fixed on the surface of some large transparent refractive cells, globular, and with a very thin wall. These are called Haimean bodies, and they rest on a layer of large cylindrical cells, refractive and transparent, and without any threads within—the Rötteken bodies. Finally, these rest on a mass of granules, amidst which are the irregular-shaped cells whose nervous function has already been suggested.

The bacilli act as elongate lenses, the Haimean bodies as transparent media, with a different refractive power; the Rötteken cylinders resemble the cones of the retinas of higher animals, and they are in the *Actinia* huge modified nematocysts.

Between the radial surfaces of the large refractive Haimean cells, and between those of the long cones, there is the same granular matter as is observable between the bacilli. This is the colouring matter, and it is opaque to light. Consequently, when the upper part of one of these turquoise-looking masses is cut off, floated, and placed reversed on a glass slide, and transmitted light is allowed to pass through under a low power of the microscope, the rays come up through the long axes of the bacilli, through the globes of the thin-walled cells, and through the long cones; but each ray is separated from those passing along the neighbouring series of structures by the presence of the opaque granules. Each series is like a little eye, and yet is incomplete as an ocular apparatus, for the nervous element is reduced to its minimum. These structures, one and all, are but modifications of the dermal tissues, and they foreshadow the more perfect instruments of the Insecta. The light can get a little deeply into the structures of the Actinia; and therefore it is possible that darkness and shadow are distinguishable from light and sunshine. The chromatophores surround the tentacular ring, and the filiform prolongations of their nerve-cells, although they do not inosculate, keep up, as it were, a connection, so that a nervous cord may be said to encircle the animal. The other traces of nervous structure are found amidst the layers of the muscles of the base. Extremely sensitive to touch, very disposed to move, and occasionally seeking another element and returning to its original one, how much of its powers are automatic and how much volitional? There appear to be the very rudiments of consciousness—of a knowledge that something must be done to ensure safety and to permit of the carrying out of the ordinary physiology; but still most of the movements must be considered as automatic.

Higher in the animal scale the relation of the presence of special sensory organs and volition becomes more decided, but it is a remarkable fact that during the life-cycle of many Invertebrata, especially if it is complicated by metamorphosis, volitional efforts may characterise early, and an automatic life the last stage of existence. The Rotifera afford examples of the development of the eye as a special sense, and they have movements which cannot be admitted to be simply automatic; again, the Balani have an active early stage of existence in which volitional action certainly plays a part, but they have a retrograde metamorphosis, and it is hard to believe that in their fixed condition there are any other than automatic movements.

In the Insecta proper, the organisation has proceeded, and amoeboid movements are rather exceptional; still the muscular fibres of the stomach and intestinal canal are not far removed

from those of the *Actinia* in organisation, and they, of course, are subject to involuntary and peristaltic movements—the goal of amœbism. In the *Insecta* the striated muscular fibres often exhibit their histological elements to perfection, and also their relation to the nerves which supply them.

In studying this interesting part of histology it is as well to keep in view the nature of the musculo-nervous cells of *Hydra*, and the muscular tissues of the animals already mentioned, for the striated fibre and its nerves are superior developments of them. If some striped muscular fibres be taken from a part of an insect which only acts occasionally, and evidently not automatically, they will present illustrations of Schäfer's views; and, if others be taken from parts which are not within the scope of volitional impulse, the transition from the involuntary fibre to the striped voluntary fibre can be observed. The pylorus of the cabbage caterpillar is not an organ affected by the will, and the majority of its fibres are long, unstriped, and are subject to peristalsis; but there is a layer of large fibres in which there is a rough moniliform arrangement. The fibres consist of an external cell-wall containing homogeneous or slightly granular cell-contents; this may be called "ground substance;" and at definite intervals this substance is infiltrated with granules which have a tendency to arrange themselves in longitudinal rows and to swell out the fibres in their neighbourhood when contraction takes place. Between these sets of rows there is only "ground substance," more or less transparent. If the muscles at the root of an ovipositor of the sawfly, an organ used with very complicated movements and intelligence, be removed and placed in Canada balsam, they exhibit the ground substance throughout. But, instead of having large localised bundles of granular substance as in the pyloric tissue, the ground substance shows alternate striæ of light and darkness, and which are produced by an arrangement of which that of the pylorus is the foreshadowing. For the longitudinal rows of granules are more pronounced, and they are cylindrical and long in the dark striated parts, and end in globular terminations in the light and apparently homogeneous intermediate zones. Hence there are two transverse rows of globular and light-transmitting rod ends in the clear spaces, and corresponding rods in the dark. The method of nervous supply to this elaborate structure is by no means settled; and, in endeavouring to estimate the relative truth of the different explanations, it must be remembered that the reagents employed by some histologists in order to render manipulation easy and the nerves distinct, are more or less destructive of clear and faintly granular, simple organic matter. Doyère, in investigating

the muscular structure of those Arachnida called Tardigrada, found that the nerve applied itself to the muscular fibre by means of a conical enlargement, and as in these animals these structures are without sheaths, the nervous and muscular elements are in direct contact. In the water beetles the nerves break up into numerous branches and spread over the muscles, and finally the sheath of each terminal nerve becomes continuous with the sarcolemma by means of a funnel-shaped process. The nervous matter passes down this process and divides usually into two branches which are applied to the sarcous elements. In the green lizard the ultimate nerve-fibril forms a conical elevation on the muscular fibril, and the axis cylinder passes through the conjoined nerve- and muscle-sheaths to form an expansion on the sarcous elements, but separated from them by a layer of nuclei and granular protoplasm. Beale, on the contrary, insists on the non-organic connexion of nerve-fibre and muscle-fibrils, but admits contact through extremely fine ramifications. It must be admitted that the description just given of the relations of the nerves and muscles of the Tardigrades, the water beetle, and lizards tallies with an evolution of structure of which that of the Hydra is the simple form.

The formation of bone in the Vertebrata, of shell and calcareous coral in the Invertebrata, are foreshadowed in the lowest animals, and in the indefinite group between the two kingdoms. In the abstract these phenomena refer to the introduction of inorganic matter and its secondary assimilation and deposition. In some of the simplest forms of living things there is an assimilation leading to a deposition of inorganic matter in the granular and in the crystalline form, for instance, in *Amœba* as discovered by Wallich. But in some Rhizopoda the introduction is purely mechanical. Thus the protoplasmic masses dredged up in the North Atlantic, north of Scotland, and where the sea floor is strewn with siliceous sand and rock, are found to have covered themselves, and even to have included in their masses minute siliceous granules and minute stones. When living in this mechanically formed case the pseudopodia are pushed out in the interstices; and when dead the organic matter perishes and leaves the investment. The accumulation proceeds by simple rolling, but some of these Foraminifera choose stones or grains of one colour for their coat, and it must be admitted that this is a profound mystery. But the other Foraminifera and Spongida take in food and out of it eliminate carbonate of lime or silica as the case may be, and deposit it regularly so as to form an external shell or skeleton. It is as much a vital process as is the corresponding formation of shell or of bone.

In *Globigerina* the sarcode deposits excessively minute grains of carbonate of lime in hollow perforated spheres and the sarcode permeates here and there. In any coral dilute hydrochloric acid will reveal a ground substance of sarcode permeating the granulo-spiculate calcareous skeleton in every direction, and the same thing may be observed in shells up to a certain age. But this does not appear to be the case in the animals which deposit silica within their bodies. In the siliceous sponges the spicules may be long and symmetrical and they are not permeated by sarcode; they are surrounded by it and the special ciliated cells of the sponge spring from them. In the *Polycystina* the exquisite siliceous skeleton is simply covered by the streaming sarcode, lumps of which are in some instances to be found on the tips of the long processes. In the *Foraminifera* and in the coral the carbonate of lime is not derived from imbibition of the sea water alone, but from the digestion and assimilation of the calcareous shells of minute animals, and although the kinds of organism are not exactly known on which the siliceous *Spongida* and *Polycystina* feed, still analogy would suggest the inference that they must have siliceous tests like the *Diatomacea* which sometimes fall a prey to them. The quantity of carbonate of lime in solution in sea water is very small and that of silica is still less, and therefore the imbibition theory which relates to these minerals is valueless. But sulphate of lime exists in considerable quantities in solution, and although it can only be inferred that it is assimilated to a certain extent by the animal, it is tolerably certain that it furnishes the carbonate of lime to the calciferous plants, such as the *Corallines*, which in their turn, being eaten by *Mollusca*, yield carbonate of lime to them. The sulphate of lime enters the delicate superficial cells of the calciferous plants in the soluble form, and is decomposed; the sulphur element goes to form albuminoid cell matter, and whilst some oxygen combines with the carbon particles in the cells, the rest escapes as bubbles of gas. The carbonic acid, part of the result of the union of the carbon and oxygen, unites with the lime and produces an insoluble carbonate of lime in the form of grains. These are found in the cell wall, in the layer of protoplasm just within (primordial utricle) and in the intercellular spaces. The most superficial cells have very little lime in them, and it does not appear except in minute quantities, where growth is rapid. These are the parts browsed upon by the smallest marine *Gastropoda* during their extreme youth, and hence the carbonate of lime of their shells. In the *Mollusca* there is great diversity of the arrangement of the carbonate of lime of the shell whether it be external or internal, and whilst the hard shell is invariably



deposited in relation to membrane its patches often assume a crystalline character in the shape of prisms or rhomboidal or hexagonal forms. But in all these if the specimen be decalcified ramifications of the membrane or sarcode in the interstices of which the granules or crystals are deposited are found; they foreshadow the organic basis of the more complicated bony structure of the lowest vertebrates or the scales of some fish where the rudiments of lacunæ and Haversian canals exist.

If there is truth in the theory which connects all animals by heredity, such an important function as that of the emission of light by the lower Invertebrata should, of course, be represented by some potentiality in the Vertebrata. Phosphorescence is, of course, a misnomer; and the term luminosity is best for the phenomenon, photogenic being the proper term for the structures implicated. A wonderful phenomenon is the emission of light by organisms, and only second to that of consciousness in interest. It is distinctly separable from the luminous appearances produced during the decomposition of vegetation and of certain animal matters, and is evidently related to several vital phenomena in which an organic chemistry induces such changes in the movements of the molecules of living matter that light is produced. The matter thus influenced is more or less fatty in some animals, but in others it stands related to the muscular tissues and to the nervous elements. The luminous *Medusæ*<sup>1</sup> and *Berœe* have an amount of fatty matter in the cells of some part of the epithelium of the umbrella, and this is luminous both in its normal position and when it has escaped into the sea. The *Sea-Pens* are in some instances remarkably photogenic; and *Panceri* states that the light emanates exclusively from the polypi and zooids. Their photogenic organ consists of eight cords, which adhere to the external surface of the stomach, and are continued into the buccal papillæ. They are composed of a substance which is contained in cells which has all the characters of fatty matter, and does not decompose immediately after the putrefaction of the polypus. A granular mineral substance, but which is neither a carbonate nor a calcareous phosphate, gives a white colour to the cords, and can pass up into the tentacles readily. Again, a general streaming movement of the cords and of the moveable protoplasm of the surface occurs, and it is attended with the evolution of light. The luminosity is produced at any spot by direct irritation, but a stimulus to the base of the animal will cause a gradual streaming movement of the light and its occurrence in distant parts. In order to understand this transmission of effects, it is as well to study *Noctiluca*, in

<sup>1</sup> See article, "Photogenic Structures" in 'Micrographic Dictionary,' Third Edition. By the author of this communication.

which singular animal the phenomena has been carefully observed by Allman and others. This animal has an irregular hollow in its sarcode, which is the stomach, and it has a tube leading to, and another away from it. Around the mass is a granular gelatinous substance, with and without fibrillation, and this ends in a mesh-like expansion beneath the transparent, homogeneous, dermal structure. There is streaming movement in the granular mass and in the mesh-like expansion, amœboid in its nature; and it is in these parts that the light is emitted in repeated flashes here and there, and not universally. It is thus that the streaming of the Sea-Pen's protoplasm is attended by the evolution of light, and anything that increases the one enhances the other. In these instances the emission of light is attendant on a generalised amœboid nervous condition. With increased evolution of the structures the photogenic parts become more specialised, and they are limited to particular groups of cells, as in some of the genus *Pholas*. These *Conchiferæ* are often highly photogenic, and their whole surface glows; but on washing off a thick mucus, which is luminous, the true organs are discovered. There is an arch of tissue corresponding to the superior edge of the mouth, which reaches to the middle near the valves; there are also two small triangular spots placed at the entrance of the anterior siphon, and two long parallel cords on the same organs. These parts stand out in relief, are opaque white in colour, and are elevations of the subcuticular tissue, covered with a special epithelium which secretes the photogenic matter. The cell contents are granular, and their colour is indefinite; the walls are readily burst, and a white substance comes forth, which contains oil-globules, and these matters are wafted over the animal by its cilia, and the general luminosity is produced. The nervous cords of the animal are only remotely connected with the production of this substance, which is soluble in alcohol and ether. In the *Ascidians* the nervous ganglia are more or less in relation to the photogenic structures, for the particular spots or small bodies which Huxley described as "cell masses," without deciding on their office, and which are made up of non-nucleated fat-bearing cells, are situated over the ganglia, but no thread or cord unites them. The matter of these cells resembles in its histology and physical and chemical reactions that of luminous organs of the animals already mentioned. In *Phyllirrhoe bucephala* an advance is made, and the photogenic matter is clearly connected with the nervous system. It is found in the common peripheral nerve-cells and in those of the central ganglia, and also in special external cells, which contain a yellow matter, soluble to a great extent in alcohol and ether. In the land *Insecta* a further

advance in construction brings the specialised photogenic organs within the range of the nerves, the respiratory, and the generative functions, and yet there are relics in them of the foreshadowings of the more simple luminous animals. Thus, with regard to this last assertion, it is found that the egg of some species of *Lampyris* is luminous, and evidently so from the secretions which covers it; and in some species the whole of the young larva is so likewise. With regard to the larger larvæ and the mature insects, it must be observed that the luminosity is not confined to the females or to the perfect beetle. The photogenic organ of the larva is on the lower and lateral surface of the last segment but one of the abdomen, where there are two bright points, connected with little sac-like bodies seated under the skin. Each sac is a gigantic cell, and has a membrane which includes granules and a fluid, and which is covered by a network of trachææ and nerves. The sac is luminous when dissected out, and its contents remain so for a while, provided they do not become dry and have plenty of fresh air. In the mature female there are luminous organs in the three segments of the abdomen preceding the last, and their structure is that of thin leaflike masses of white colour, placed beneath the skin, and they are well supplied with trachææ and nerves. Each leaf or lamina is composed of groups of cells, each of which has its trachea and nerve. The cells contain granules and nuclei, and the whole are close to the great nervous masses and the generative organs. In the adult the glow occurs during the period of the greatest excitement of those organs. Instead of the streaming there is localised nervous effort; instead of the direct action of amœboid matter there is conducted action; and in the lowest of the luminous, as well as in the highest of them the fatty matter and albuminoids of the tissues, general or special, as the case may be, suffer oxygenation, and light is evolved. Light the foreshadower of animal heat in other organisms, and perhaps of the evanescent rosy tint on the cheeks of the most beautiful in creation.

The simple shapeless mass of protoplasm in which even vacuoles are not seen, and which Hæckel has designated *Protophytes* reproduces itself by fission. A contraction occurs which increases and the mass becomes two, each of which has an independent existence. This is the foreshadowing of the apparently essential cleavage of the ova of the higher animals, the ovum being the repetition of the simplest form of life. The next advance in the reproductive process is seen in such creatures as *Protomyxa* which Hæckel described from Lanzerote in the Canaries. A yellowish sarcode amœboid and streaming in the fully developed animal which after a time becomes motionless, when the outer parts acquire the nature of a homogeneous cell

wall. This wall increases in thickness and encysts the *Protomyxa*, and then a vast number of pear-shaped bodies are formed by splitting up of the protoplasm of the interior: these escape and a strong flagellum arises from their pointed end with which they propel themselves through the water. After awhile this cilium disappears and the creature crawls like *Amœba*, and becomes the fully developed *Protomyxa*. In this instance several of the zoospores may unite and fuse together to form the perfect form. In the life-cycle of *Magosphœra* there is an encysted (resting) stage, then the protoplasm cleaves and subsequently the cleaved masses become amœboid and swarming takes place; the masses being ciliated before the final amœboid stage is attained. *Amœba princeps* becomes encysted and minute mobile bodies escape, each to become something like *Protamœba* and then to resemble the parent form. Some of the Protozoa which have nuclei lose them just before the cleavage stage of their encysted protoplasm. Thus in *Gregarina* the body becomes motionless, spherical, and encysted; the nucleus disappears and the tissue of the animal breaks up into a number of globular particles. This is very suggestive in relation to what happens in the animals when true ova are produced, and especially when it is remembered that amœboid movements of the yolk-corpuscles take place even in Vertebrata.

The fusion of several zoospores of *Protomyxa* to form one mass, which becomes the perfect animal, foreshadows the conjugation of such Rhizopoda as *Noctiluca*. In this genus two animals touch by their surfaces close to where the tube leads from the stomach and their tissues presently merge into each other. The so-called nucleus of one unites with that of the other and cleavage of the whole united mass ensues, the cleaved masses developing into perfect *Noctiluca* which then escape. In the Infusoria there is the first evidence of the union of two elements in the production of a fertilised ovum and all the attendant phenomena, and although they are in advance of those of the reproduction of the animals already noticed they clearly refer to them. Balbiani has described the process of reproduction in *Paramecium aurelia*, and has shown that each infusorium contains male and female organs, and that at certain times two of them join, their similar extremities being turned in the same direction and their mouths closely applied. They move round and round their axis in this position for five or six days, during which important changes take place in the condition of the reproductive organs. Each individual contains an ovarium, having at first a smooth surface, from which proceeds an excretory canal or oviduct that opens externally at about the middle of the length of the body into

the broad fissure or so-called mouth. Each individual also contains a seminal capsule in which is seen lying a bundle of spermatozoa curved upon itself, and which communicates by an elongated neck with the orifice of the excretory canal. It may be mentioned here that these spermatozoa are the result of cleavage of the protoplasm of the so-called nucleolus. The next process is the lobation of the ovarian and that of the spermatid mass, the cleavage (which is repeated) of the one producing ova and of the other more spermatozoal masses. Then these seminal capsules previous to their perfect development pass from one Infusorium into the body of the other, and subsequently the spermatozoa come in contact with the ova. The contact of the spermatozoa is of the same kind as in *Spongilla* and in the case of all the higher animals in which it has been witnessed.

In the higher Cœlenterata the granular ovum cleaves and subdivides, and the separate masses have more or less individual vitality and movement, but they do not swarm and separate, but conjoin, and the involution of the top side produces the rudimentary endoderm and stomachal cavity of the Planula stage which has been of late named Gastrula. It is, indeed, remarkable that whilst unicellular animals should have cleavage, each separate part becoming a separate animal, that in the multicellular the cleavage should go on and combinations of the cleaved mass should develop the compound organism.

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#### X.—Macalister on Animal Morphology.<sup>1</sup>

THIS is a most excellent book, and Professor Macalister deserves great praise for its elaborate production. He makes no claim to originality, and would have it believed that the work is a compilation from the best and most advanced sources of information; but it is only fair to credit him with having placed all this knowledge in a very commendable form, and in a most judicious manner, before his readers. In fact, the book could only have been written by a very accomplished and industrious naturalist, from whom science may expect some good and lasting original work.

It is as a work divisible into six parts, of which the first part treats of the simplest form of living material and of the cell, and the second relates to general form, structure, and arrange-

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<sup>1</sup> 1. *An Introduction to Animal Morphology and Systematic Zoology*. By A. MACALISTER, M.B. Dubl., Professor of Comparative Anatomy and Zoology in the University of Dublin, Part I.—Invertebrata. pp. 461. Longmans, 1876.

ment, or general morphology. The histology of epithelial and connective tissues, and of muscular and nervous tissues, is contained in the third part, and the fourth considers the nature and distribution of organs. The fifth part refers to reproduction, species, types, and parasitism; and the rest of the work relates to the distribution of animals, classification, and the details of the Invertebrata.

As it is a book which concentrates nearly all the advanced opinions and terminology of the great teachers of natural history, and which abounds in excellent descriptions, it should be in the library of every medical man, and of every advanced student of the sciences which Professor Macalister teaches.

In thus bearing testimony to the good qualities of this much wanted work and to the accomplishments of the author, we acquit ourselves of a just and pleasant duty; but there is another to be performed—that of criticism of what has been well put together, and of a few opinions, and its performance is necessary in the interests of science. In doing this it must be thoroughly understood that nothing which may be noticed in a vein of censure reflects on the author.

Firstly, then, the author has written too good a book for those for whom it was primarily intended. As a teacher, Professor Macalister felt that his students required a text-book, and this is the result. It is far beyond the reach of most students, and may rather take its place on the bookshelves of the naturalist as a work of reference and instruction. It is clear that junior students are expected to restrict their reading to those parts printed in large type, but even these portions are too advanced unless carefully explained by the teacher, and unless the student has had the advantage of a much more careful preliminary education than is usual. Certainly, if the book is to be used amongst English students, the University of London had better restore Greek to its former position in the matriculation examination; for as the terminology of a great natural history school is now full of sesquipedalian Greek words, it is from the nature of the book thoroughly represented. It is no fault of Professor Macalister that simple nature is presented to the student, by the great luminaries of the day, in language as complicated and involved as possible; or that the dominant school will have a Greek term for everything, whether it can be easily expressed in good English, or German, or French. A definite and correct nomenclature and terminology is absolutely necessary in natural history, but word-coining may have its demerits, and has positively at the present time become a thorough nuisance. It replaces original research in some naturalists who have done a little good work,

and to "throw about the dictionary" is to them as satisfactory as to labour with scalpel and microscope. There are some men whose scientific career has been wonderfully assisted, in the eyes of the unscientific, by the evolution of Greek jargon, for the uninitiated think that a hard word is as good as a stern fact. Hence there is a great temptation to produce new terminologies for old ones, and, as a rule, the word is thought quite as important as the idea it represents. This objectionable elaboration of terms is not confined to the higher branches of morphology, for it prevails in the classificatory sciences. Every naturalist of the new school must indulge in a new classification and in new terms—of Greek origin, of course—without much, and often without any, regard to the excellence of those of his predecessors in the particular line of research. Doubtless there is great merit in the simplification of terms and expressions by a happy word, Greek or Latin, and most of the very distinguished naturalists of the day have advanced the cause of learning by coining them. Such words are of good and current value, and must be disassociated from the flashy, spurious medium. Nevertheless, it is too true that the ability to produce what is good is, in this case as in most others, accompanied by the temptation to do the reverse. Haeckel probably outdoes his brethren in this unhappy peculiarity, and as Haeckelismus is a prevailing complaint, most juvenile naturalists impressed with the stupendous veracity and overwhelming superiority of everything German use their Liddell and Scott freely.

Plain English and even easy terminology will have their day, and will last long after Haeckelism and all other natural science Philistinisms have passed away. But at present the German-Greek epidemic is at its height, and, indeed, may be said to be brought to a focus by Professor Macalister. Take an example by turning to the definitions of some subkingdoms and orders. Suppose a student or a medical practitioner, who is fond of his microscope and a little easy aquarium or pond work, wants to know something about the Hydra. He knows that his fathers called it a polypus, and that a great German seeing, what every one had known for half a century, that it had a hollow cavity in the body with only one opening, the mouth, had called it one of the Cœlenterata. The chapter on Cœlenterata is looked for and found (p. 79). There stands subkingdom Cœlenterata (Leuckart). But there is a note to explain that the term is equivalent to Diploblastica, called so in contrast with the Ablastica and Triploblastica; and we are informed that Ablastica and Protozoa mean the same, and that all the other Metazoa, whoever they may be, are included in the Triploblastica. This little difficulty having been accomplished, the reader will find

that the Cœlenterata and their aliases are "aquatic radiated personæ (often in colonies), of more than two antimeres, having a body cavity opening at one pole by a mouth often surrounded by feelers (tentacles)." Probably if the Medicus loves Falstaff he will quote, "Think of that, Master Brook," and the student may employ other expletives. In page 81 comes some more, and there is a woodcut of *Hydra aurantiaca*, and the readers are told, "1. *Eleutheroblastea* (Allman). Hydraz are separate personæ, attached at will by an aboral disc and consisting of a tubular digestive cavity with no anus. The 4—10 (rarely 12 or more) tubular tentacles on prolongations of both endo- and ectoderm vary in length from one quarter to ten times the length of the body." "The ectoderm consists of large nucleated cells, from whose bases filamentary processes are continued inwards. Kleinenberg regards them as nerve-cells, and the processes as muscular, thus forming the simplest differentiated neuro-muscular apparatus. Between those cells are smaller, irregular, interstitial cells, not forming a special layer, but surrounding the bases of the first series, and containing the little cells, which often appear sunk in the sides of the larger ectodermal cells. Fine bundles (palpocils) project from the walls of some of the surface interstitial cells: protoplasmic processes of T. S. Wright. The endoderm cells are large, unilaminar, vacuolated, often flagellate, often with no distinct cell-wall. The thread-like processes of the ectoderm cells, united by a copious intercellular substance, form an intermediate layer of vertical fibres (described by Kolliker). Amœboid cells wander through both layers."

Now, this is a fair specimen of modern biological didactic writing, and it certainly makes one long for the more comprehensible flowing English of the authors of the 'Cyclopedia of Anatomy and Physiology.' It requires a great deal of study to comprehend the above. Firstly, Allman's "*Eleutheroblastea*." We suppose that amongst the Hydroida it is the name of a group—an order. *Eleutheros* means free, and *Blastea* is a bud, sprout, leaf. It would answer the purpose if the order was called that of the Free Hydroida. But what are "Personæ"? They are described page 11, which we quote also in explanation of the word "antimeres" mentioned above:

"The simplest animals are plastids or homoplastic aggregates. In the higher forms greater differentiations occur, and when heteroplasts are grouped into organ-systems these are generally symmetrically arranged, similar heteroplasts being placed side by side around a horizontal axis. Thus, a star-fish consists of segments or groups of organ-systems, each built upon the same plan, placed radially around a centre. Such segments may be called *antimeres*



(Haeckel). They vary in number; each segment of a bilaterally symmetrical animal (vertebrate or anthropod) has two; some Radiolaria three; Medusæ four; many Echinoderms five; Zoantharia six; Luidia seven; Ctenophora eight, &c. The increase of a function may be provided for by a multiplication of organs, or a more complicated development in those already existing. Of these methods the former seems the simpler, hence the next step in complexity is the formation of a chain of similar groups in succession. To each in this chain is given the name *metamere*. These segments may be similar, as in the case of the zonites of a worm, or else some may be specialised. The whole chain constitutes a *persona* in which the metameres are usually successional (catenated), or else lateral (fruticose). The highest degree of aggregation in the animal kingdom is the *colony* in which a number of *personæ* are united on a common stem, as amongst the compound Hydrozoa."

This is certainly not milk for babes, but very tough meat, whose digestion really produces no nourishment to the understanding. It is an unmitigated, useless complication of very simple matters of fact. It is a dressing up of well-known matter in involved and singularly infelicitous language.

To proceed: we are informed that the Hydra-Persona has an aboral disc. It has really no proper disc, but it can spread out the part of the body opposite to the mouth and hold on. And with regard to what follows in the description quoted, it must be remembered that the researches of Kleinenberg were made on Hydra whose tissues were chemically altered by reagents—made as much like nature as a boiled egg is to one recently laid. Nevertheless, the muscle-nerves are good facts, and it is a pity the author did not state that the muscular layer is between the ecto- and endoderm. It will be noticed, however, from the section of Hydra given by Kleinenberg that the muscular layer is part of the ectoderm. The author is quite right in his caution that little cells appear to be within the larger cells; they are not, and it is a mistake of optical effects which has induced some microscopists to be of the opposite opinion.

Few objects are more readily obtained and studied than the Infusoria, and we turn naturally to the chapter to see what new light modern research and nomenclature have thrown on them. At the onset we find a name attached to Infusoria (Wrisberg) as its author, and as he is thus exact we are disappointed in finding the other names appended to the descriptions of this well-known class, not by any means representing what has been done. Stein, Leuckart, Kühne, Mecznikow, Cohn, Perty, Eberhard, Claparède, and Lachmann, and Haeckel, are the names, and we look in vain for those of Pritchard, Griffiths, Dujardin, Carter, R. Jones, Balbiani, Lieberkuhn, Ehrenberg, Huxley, Gosse, Drysdale, Wallich, Archer, &c. The

authors mentioned in the book who have done great work are Claparède and Lachmann and Cohn. No one believes in Stein, and Kühne merely takes up common knowledge and makes a discovery of what has been known for years. The Infusoria are stated by our author to be minute ciliated bodies, probably simple plastides, or at most cytocormi, with a firm chitinous cuticle, an ectosarc and an endosarc, a contractile vesicle, and a nucleus. Now, every one who has had much to do with the class will be dissatisfied with this definition. A plastid is an independent mass of protoplasm, and when cells fuse together within a common investment they constitute in modern jargon cytocormi; but there are strong arguments in favour of the multi- as well as of the unicellular nature of the Infusoria. A plastid proper is without a cell-wall and has no nucleus; it is the new name for the protoplasts of English authors, and such simplicity is never found in the fully developed Infusoria. The body of the Infusoria has not invariably a firm chitinous cuticle. The outer layer of the body evidently adheres in some species and is perfectly soft, and in almost all there is a delicate layer without anything like chitin in it; it is pitted by the cilia and is a secretion of the sarcode beneath. Besides this many have a carapace or lorica, which is often hard and very distinct from the other layer, but the word chitinous is not that which should be used to represent its peculiarities. The term ectosarc is to replace the old term cortical layer, and the endosarc the central sarcode, and with doubtful propriety, for the new terms tend to the improper and impossible separation of the inner and outer parts in the histological sense. The layers are not worthy of the name and the mass is continuous.

In noticing the cilia Professor Macalister states that "sometimes an undulating membrane takes the place of cilia (*Undulina*, *Pleuronema*)."  
*Undulina* is a new name for the old bit of membrane called *Trypanosoma* by Grube, and probably it is more correct to connect some of the parasitic *Trichodinæ* with undulating membranes than *Pleuronema*. Most observers will demur to there being "a contractile striped material, possibly a simple form of muscle," in *Stentor* and *Prorodon*, or that these striæ "are spiral in *Spirostomum*." It appears that in the last the spirals are external to any contractile matter, and the appearance in the first-mentioned Infusoria is owing to markings which have no relation to fibre but to ordinary contractile sarcode. The author mentions, "The contractile stalk of *Vorticella* has also a highly refracting muscular axis (Kühne; its muscularity is denied by Meczni-  
kow, as it is structureless, myophanic). The outer layer of

this stem is cuticle, and it strengthens the stalk by its elasticity." The nature of the contractile stalk was settled and became the common knowledge of every microscopist long before the men whose names are mentioned were born. There is a central fibre which is continuous with the protoplasm in the base of the bell of the infusorian. It is faintly granular and contracts like an involuntary fibre, or like the tentacles of *Actinia*. Call it what you will, it acts as a muscle and therefore in common sense is one. The cuticle is elastic, as Professor Macalister suggests. The digestive apparatus is well described by the author, but it appears better to state that the body of *Trachelius ovum* consists of an internal part whose protoplasm is discontinuous, and allows water and food to penetrate it in many directions, than to assert that it "consists of an endosarc of protoplasm threads forming irregular meshes whose interspaces are filled with water." The author says the threads remind one of *Noctiluca*. This is straining an analogy, for in this interesting animal, which, by the bye, the author strangely places among the Flagellata, the mouth leads by means of a tubular gullet to an irregular hollow in the interior of the animal's protoplasm, and this hollow or stomach has an anal opening. Around the stomach is a granular, homogeneous, glutinous substance with indistinct fibrillation, and this ends in a mesh-like expansion beneath the skin—the seat of amœboid movement and of luminosity.

The author divides the class into four orders: 1st. The *Opalinæa*. It is hardly advisable to place this very doubtful group in the Infusoria at all, and it has been excluded in the best classifications. The author mentions *Plagiotoma* as a genus of it, but really it is a ciliate infusorian belonging to another order. *Undulina*, brought in also, we have already noticed as being of no value. 2nd. The *Peridiniæa*. These are usually called the cilio-flagellata or the *Peridinina*. The author defines the group as follows: "Mouthless (mouth-bearing, *Schmarda*), free loricated forms with a transverse ciliated furrow, no contractile vesicle, but with a nucleus, vacuoles, and chlorophyll granules. Some have a red pigment spot (*Glenodium*); others have two or three long-eared horn-like processes (*Ceratinium*). The lorica is often double, chitinous (or siliceous?)" "They unite the Flagellata and the Infusoria." Here are, then, most doubtful beings classified with the 2nd order. Some are without a cuirass and are naked, and *Glenodium* is an alga. *Schmarda*'s division is probably incorrect, and that of *Claparède* and *Lachmann* should be adopted. In this last form of classification the order is divided into those with and without a transverse groove, and even in its genera there are doubtful forms.

3rd. The Acinetina. These are the Suctoria of Claparède and Lachmann, by far the most advanced authorities, who are moreover free from the rampant errors of the day. They are in some instances worthy of the name of perfect animals, that is to say, their last stage of existence is a pedunculated mass with retractile processes or suckers: but most of the genera and species belong to other than infusorians, are larvæ of Infusoria or are lower in the scale, and are Rhizopoda. We feel inclined to place the true Acinetina even amongst the Rhizopoda. There is a remark made by our author, "Eberhard saw one form becoming ciliated and changing into *Bursaria truncatella*, that is, with a high class true ciliate infusorian." We should prefer stating that the immature *Bursaria* resembled an Acinetina. Most investigators of the Infusoria will agree with us that Carter's descriptions of the development and regression of the *cilia* and suckers in the Acinetina explains the supposed metamorphosis. If Carter's name had been Kärner, doubtless his admirable work would have received more notice.

Prof. Macalister's fourth order is that of the Stomatoda. This name is to replace the old-fashioned and better term Ciliata. This new order is divided into four suborders:—"1. Peritricha, with a ciliated peristome, including five families, the Vorticellidæ, Trichodinidæ, Ophrydinidæ, Dictyocystidæ, and Ophryoscoleidæ. 2. Holotricha, uniformly ciliated, with seven families, i.e., Cyclidinidæ, Eichelidæ, Chelodontidæ, Colepidæ, Trachelidæ, Ophryocercidæ and Colpodidæ. Then the third order has a second longer set of cilia near the mouth and fine cilia over the body. Heterotricha has two families, Bursaridæ and Stentoridæ. Finally, the fourth order, Hypotricha, is only ciliated on the under side. It has four families, Oxytrichidæ, Euplotidæ, Dysteridæ, and Aspidicidæ. It will strike the reader that, although this classification depends on the distribution of cilia, the great order is still not termed Ciliata, and Claparède and Lachmann's well-known term is gratuitously superseded.

If this very complicated classification is analysed it will be found infinitely less philosophical than that generally used.

The Trichodinidæ are separated from Vorticella, and have Halterina included. Now, the division of the Vorticellina into those free and attached is good because the attached can, when broken off, live free; and into those with and without a ciliary crown posteriorly is equally good. Hence the Vorticellidæ—"ina" of others—must include the Trichodinidæ. Then Halterina cannot come into this last family, and its fine leaping setæ exclude it and place it in a family by itself. Again, the Ophrydinidæ mentioned—Ophrydrium, Vaginicola, Tintinnus,

and Cothurnia—are all good allies of Vorticellinæ, and we do not see the advisability of subdividing. The fourth order is probably not infusorial at all, and the fifth is not sufficiently defined, so that it is really advisable to put the old Vorticellinæ as defined by Claparède back again in the place of all these five useless subdivisions. Space will not permit of a criticism of the author's second and third suborders, but a little examination of the fourth will show that genera are made into families, that families which cannot be related are included in the same group. The Oxytrichidæ cannot be separated from such forms as Euplotes and Aspidiscus, and here Claparède's classification is the best. We doubt the propriety of the placing Dysterina apart from Trachelina and Colepina as families, and therefore demur to the position given by the author to the Dysteridæ.

Every naturalist of experience is desirous that the synthetic method should prevail under existing circumstances. It is the necessary mode of thought in the cultivated intellect of a good zoologist. In early days the student compares and sees differences, and therefore he is disposed to increase the number of species, genera, and families, and to confound the idea of the genus with that of the species; but with experience comes the desire of doing the exactly opposite, and of linking together forms, making the genus larger and families fewer. Hence the author will certainly have to teach his students that the Infusoria as a group require more simple treatment than they have received from all others excepting Claparède and Lachmann.

Professor Macalister, although very devoted to Haeckel, does not retain the kingdom Protista which that naturalist raised up to represent in words the debateable ground between definite animals and definite vegetables. He says, "But most of these" (Protistæ), "though not falling in rigidly with the definition of either, have recognisable affinities pointing to one group or the other, and as the limits of every class are arbitrary, it is easier to draw one line of demarcation than two; hence I have not adopted this arrangement."

But are there very definite lines between the animal and the vegetable in the lowest forms of life? The necessity of the distinction is not obvious, and the philosophy of the age is opposed to hard and fast lines. Are there not beings which at one time of their life lead an existence which we call vegetable and at another animal? Certainly, and even in such groups as the Fungi and Lichens, the kingdom has been considered to be of value by most teachers, and possibly our author may reconsider his decision.

The Brachiopoda are well described, and the terminology is free from the prevailing complexity, but we demur to the right

to alter such well-known terms as adductor, cardinal muscles, and the introduction of "occlusores" and "divaricatores," to say nothing of "adjustor ventralis." The author, however, gives us some information which had escaped us, to the effect of the deficiency of a heart in *Lingula*, and that the circulation is carried on by cilia in large vessels (Semper). He also states the interesting fact that some young creep by means of a pair of spines, one on each side of the ventral mouth lobe.

The bivalve and univalve Mollusca are very carefully done in this work, and the amount of labour and trouble they must have cost must have been excessive. The Insecta have their internal anatomy admirably described, and their classification is founded on the alterations of form which prevail during the life cycle. This is a fair method; and were it not for the fact that some insects, closely allied in every structural respect, have different kinds of or no metamorphoses, it would be as good as any scheme. The illustrations are fair in the volume, many are new and good, and the curious *Apocrypta paradoxa* comes the last. It would be as well if the cut were turned up, or that an arrow were placed on it indicating the head. A good index is found at the end of the volume, and in future editions a glossary will be found indispensable.

After considering the vast amount of labour entailed in the production of this book, and its applicability to the public—for the public are disgracefully ignorant of natural history—there comes the reflection that what is really required for students and beginners is a description of the types of the classes, orders, and families. A simplified 'Forms of Animal Life,' with such a context as Professor Macalister could write, would well repay him.

## Bibliographical Record.

**Mivart's Lessons from Nature.**—The present work, as a contribution to metaphysical and biological science, is one of the most important which have appeared since Darwin's 'Origin of Species,' and we make no apology for a somewhat detailed analysis of the portions which relate especially to the modern progress of comparative anatomy and physiology. Parts of the work refer to such important subjects as the likenesses which prevail in animals and plants, natural selection, and sexual selection, and we are tempted to go at some length into the novel and striking arguments which Professor Mivart gives on these subjects. Whilst our space precludes us from a synoptical analysis of the work, we shall briefly consider parts of the eighth chapter. In this the author seeks to prove that the facts of mimicry and the various kinds of homology, as exhibited in comparative anatomy, teratology, and pathology, reveal an internal force and dynamic agency, the soul in each animal, which forms one indissoluble unity with its material frame.

In considering the form and structure of animals and plants among the different resemblances presented to our view, there are two orders of likenesses which he notices in detail. The first of these is one which is merely external; i.e., the likenesses borne by different animals to others of more or less different nature, plants to inanimate objects, and likenesses borne by plants to others of more or less different character, or to animals. This kind of resemblance is termed *mimicry*. The second of the two orders extends to internal structure, and relates to likenesses of the kind borne by parts of one animal or plant to parts of other animals or plants, and also to likenesses borne by one part of any animal or plant to other parts of the same individual.

*Mimicry* is often a close and striking, yet really superficial resemblance borne by some animal or plant to some perhaps very different object. The bee and spider orchids, or the clear-winged moth, which may be mistaken for a bee, is a case in point. One of the most perfect

examples of mimicry is the insect of the grasshopper order, which is called on account of its appearance, the walking leaf, as both in form and colour its body so closely resembles a leaf that it is most difficult of detection, when found among real leaves. On this subject the excellent observations of Mr. Bates and Mr. A. R. Wallace have thoroughly familiarised the public with many of the most salient facts. Not only moths but also beetles imitate bees. Wasps, and even such strange objects as dung and drops of dew, are also mimicked by beetles. Walking stick insects simulate bamboos, and some insects which mimic leaves, mimic even the marks made upon leaves by the ravages of other insects, or by mould. It is difficult to imagine that such facts could be explained by Mr. Darwin's laws of accidental variatives; and Mr. Darwin himself, with a loyalty to truth and accuracy which he has always felt, has frankly confessed that in his early writings he did not "appreciate how rarely single variations, whether slight or strongly marked could be perpetuated."

On this subject the opinion of Mr. A. R. Bennett of St. Thomas's Hospital may be considered as conclusive. For, he remarks, "no conjunction of external circumstances will avail to account for these cases of mimicry, whether acting through natural selection, or any other known process." It might well be assumed that the extraordinary resemblance of the flower of the bee orchis to the body of a bee was designed to attract these insects to the flower; but unhappily for this theory the bee orchis appears to be one of the comparatively small number of plants that are independent of insect agency for the maturing of their seeds. Surely for minute accidental variations to have built up such a striking resemblance to insects we ought to find the preservation of the plant or the continuance of its race depending on relations between bees and it. It seems, therefore, that these facts of mimicry reduce us to the acceptance of a belief in an innate tendency implanted in certain races of animals and plants, to assume the external semblance of creatures very different from them; a tendency the existence of which is to be explained by no mechanical conceptions, though in many instances the destructive agencies in nature must tend to keep true and to intensify such resemblances.

We may now turn to the second order of resemblances found in animals *i.e.*, likenesses in internal structure as well as external form, agreements and differences respecting which various very different explanations have been offered. The real existence, however, of the different kinds of resemblance about to be referred to as facts cannot be denied.

The speculations of the transcendental anatomists, amongst whom such names as Goethe, Oken, Spix, Carus, De Blainville, Geoffroy St. Hilaire, and Owen may be mentioned, have occupied



the thoughts of many of the past generation of anatomists. The vertebral theory of the skull in an amended form became advocated in England through Professor Owen, and anatomical science in this country will ever be very deeply indebted to him for his attempt to familiarise the English mind with "Philosophical Anatomy," since all must at least admit that it has been the cause of an important scientific advance, and to no small degree in consequence of the efforts made to modify or to refute it. It was urged against Owen's vertebral theory of the skull that if true, its vertebrate character should be plainest in its earliest and least modified stages; and that yet such stages had no resemblance to vertebræ at all. Indeed it was triumphantly shown that, as soon as the backbone begins to be a backbone, the skull begins to be something very different. In fact, that the skull is never segmented, as is the primitive vertebral column, but mainly consists, in its earlier stage, of a mass of cartilage, from which two cartilaginous rods (the trabeculæ cranii) extend forwards along the base of the brain-case, quite unlike anything found in the incipient vertebral column. Yet other suggestions were made by Professor Seeley and by Mr. Herbert Spencer to account mechanically (by the necessary action of pressure and strains on a frequently flexed, elongated cylindrical body) for the simultaneous existence of a segmented backbone and a non-segmented skull. Finally, a flood of ridicule and sarcasm was poured on the vertebrate theory of the skull, and the doctrine of archetypal ideas was supposed to be once for all disposed of by means of the hypothesis of evolution. Mr. Darwin's 'Natural Selection' was lauded as having given the *coup de grâce* to such fancies; and, lastly, appeared 'Pangenesis' to slay the slain, and to make fortuitous compounds of atoms occupy the vacant throne of the deposed prototypal divine ideas. Evolution seemed to many persons to have this destructive effect, because by and through it, similarities existing between the parts of different animals came to be represented as exclusively due to blood-relationship between them. It was no longer a wonder that the skulls of a monkey and a mud-fish were essentially similar, if both these animals were the diverging descendants of some ancient common ancestor.

Professor Owen many years ago pointed out the distinction between analogous and homologous parts; and about the same time that a series of resemblances began to be recognised, which could neither be explained on the theory of common descent, nor on that of resemblance to a primitive archetypal idea. *Lateral* symmetry is shown between the right hand and the left; *vertical* symmetry exhibited between the upper and lower parts of the tail-fin of most fishes; and serial homology, presented by a series of generally similar bones, making up the vertebral column or backbone, are likenesses which are unquestionably not due to inheritance. Mr. Herbert Spencer's ex-

planations of them by reference to their manner of environment was clearly inadequate, as it led to the assignment of external causes for operations in which no external condition could have had any influence on the embryo. It was thus established that there are likenesses or homologies which cannot be due to inheritance, and which have to be distinguished from others which are, or which may be so due. The rather inelegant words (taking the Greek derivation into account) were coined by Mr. Lankester of "homoplasmy" and "homoplast" to express such uninherited resemblance and such resembling parts, and a legitimate successor to the intellectual inheritance of Don Antonio de Armado further invented the antithetical terms "homogeny" and "homogen" to express inherited resemblance and the part which manifested it. Professor Mivart considers that the number of similarities which have arisen independently, *i.e.*, cases of homoplasmy, is prodigious, as well as that very great caution is necessary in endeavouring to discriminate between likenesses which may be due to inheritance and those which are due to some other cause. Professor Parker has demonstrated the existence of an apparently inexhaustible number of cross relations between widely different animals, which show more and more plainly the entangled independencies of their structure. Once "evolutionists" (whatever that word may mean) held the notion that "similarity of structure" necessarily implied "genetic affinity." But as a biological axiom this vague proposition cannot now be maintained by any well informed naturalist.

It is against reason to suppose that mere indefinite variations together with the survival of the fittest could ever have built up all the serial, lateral and other homologies, without the action of some innate power or tendency so to build up possessed by the organism itself in each case. There are other external evidences (besides the homologies themselves) of the existence of such an internal power, by the action of which these recondite likenesses may be conceived to be brought about. The evidence of the existence of special internal power, which evidence may be gathered from three sources. 1. Comparative anatomy. 2. Teratology. 3. Pathology. On the hypothesis of evolution tortoises must be reckoned as very far indeed from being the first and earliest kinds of quadrupeds. Yet certain tortoises exhibit the most extraordinary resemblance and correspondence between their anterior and posterior limbs. This degree of likeness and correspondence then, must be the effect of a spontaneous development, and cannot be merely due to inheritance, because it does not exist in other forms which, upon evolutionary principles, are more nearly related to hypothetical root-forms.

As to teratology, it is notorious that serially homologous parts tend to be similarly affected, great toes showing abnormalities of

structure with thumbs, and ankles with wrists, knees with elbows, and so on. Professor Burt Wilder has recorded six cases in which both the little fingers and both the little toes were similarly affected, and one case in which serial symmetry was alone affected, the right little finger and the right little toe being the only ones affected. But perhaps the most curious and instructive instances are those in which the feet of pigeons or fowls are abnormally feathered, or, as it is termed, furnished with "boots." These extra feathers are developed along the very parts of foot which correspond to (*i. e.* are serially homologous with) those parts of the birds' hands which bear the wing feathers, so that these "boots" are plainly a serial repetition of the true wing-feathers. These foot-feathers have indeed been sometimes proved to exceed the wing-feathers in length. Moreover the foot-feathers resemble the true wing-feathers in structure, and are quite unlike the down which naturally clothes the legs of such birds as grouse and owls. But there is a more striking correspondence still, for in pigeons which are thus "booted" the two outer digits (toes) become more or less connected by skin, as is also the case with the corresponding digits of the pigeon's hand.

As regards pathology, Sir James Paget has declared, speaking of symmetrical diseases, that "a certain morbid change of structure on one side of the body is repeated in the exactly corresponding part of the opposite side," *i. e.*, we have a spontaneous manifestation of lateral homology. In the pelvis of a certain lion affected with a kind of rheumatism Sir James remarked a deposit which had formed a pattern more complex and irregular than the spots upon a map, while not one spot or line on one side failed to be represented with daguerreotype exactness on the other. He also considers that parts which are serially as well as those which are laterally homologous, are likely to be affected in a similar manner. Such serially homologous parts are the back of the hand and the corresponding surface of the foot, and these are likely to be both modified in the same manner, as also are the palms and soles, the elbows and knees, together with the other serially corresponding parts of the arms and legs. Such facts are of course brought about by a "nutritional process." But this is but a restatement of the fact itself, and affords no explanation of it whatever. The question is, "what is the cause of this nutritional balancing?" If this power be referred, as it seems that Mr. Spencer would refer it, to certain physiological units of which he imagines that each organism is composed, there must none the less be recognised an innate power possessed by such units of inheriting the effects of ancestral modification. It seems more easy, more simple, and more consonant with known facts to recognise in each organism, as a whole, an innate power tending to development of a special kind, although the actual results of the

developing force must be modified by the external conditions which happen to exist in each case during the process of development. Among the results of the recognition of such innate powers and tendencies are an increased support to teleology and a rehabilitation of "Philosophical Anatomy."

From the Hunterian Chair, where Professor Owen, in 1849, promulgated his views as to philosophical anatomy, Professor Huxley, in 1870, gave out his quasi-vertebral theory of the skull. We know that the whole body of every animal with a distinct skull and backbone exists at first as a rounded almost structureless mass of tissue, in which the first clear indication of such animal is a longitudinal furrow, marking the place of the future spinal marrow and brain. In subsequent development the bones of the skull, especially in the higher animals, present a singular reminiscence of vertebræ in the three serially successive arches which they form. Certainly if the essence of vertebræ consists in there being a series of bony rings fitted together and enclosing the the nervous centres along the dorsal region of the frame, then it must be asserted that the skull is in part composed of three long vertebræ. In certain fishes the transition from the spinal column to the skull is so gradual that it is easy to mistake part of that column for part of the skull. Thus, in the sturgeon, the cartilaginous representatives of true vertebræ coalesce into one mass with the cartilaginous skull; and in the siluroid fish, *Bagrus*, the bony vertebræ next the head are greatly expanded, and join each other by the same mode of union, by suture, as do true cranial bones; and this shows how undoubted vertebræ may simulate cranial walls. It has always been affirmed that, as regards development, while the spinal column is essentially and almost in its earliest stages a serially segmented structure, the primitive skull presents no serial segmentation. It is indeed true that parts which temporarily or permanently represent in cartilage the bony skull are never serially segmented; and more than this, the cartilaginous precursors of the bones on one side may be completely separated by an interspace of softer substance from those of the opposite side, a single fore and aft segmentation in the skull thus violently contrasting with the manifold transverse segmentation of the spine. But it has recently been noticed that, in the young eel and axolotl, before the base of the future skull has become cartilaginous, an indication of transverse segmentation is to be traced in the soft tissues of that region, a proof of what oversights might be committed by relying too hastily on development as our guide. One of the most curious incidents in the history of science is the return just made ('Proc. Roy. Soc.,' No. 157, p. 127) by Professor Huxley to the conception so long ago advocated by Professor Owen, that serial segmentation, however latent and disguised, extended primitively and fundamentally to quite the anterior end of the head. We

have an approximation to the early form of the vertebrate skull in that very exceptional little fish the lancelet (*Amphioxus*), in which the front end of the body is, like all the rest of it, made up of a series of similar segments, although the part representing the bodies of the vertebræ of higher animals is itself unsegmented. The constantly increasing number of instances of the independent origin of structures like the eye-spot of *Amphioxus* makes Professor Mivart think it far from improbable, that vertebrate genetic affinity may be at least as much in the direction of the annelid worms as in that of the ascidians, and that there are hardly as yet data to determine which of the curious cross-relationships exhibited by the lancelet are due to genetic affinity, and which to homoplasy.

The conception of cranial vertebræ then, like conceptions of serial, bilateral, special and general homology, all forming parts of "Philosophical Anatomy," are subjective apprehensions of relations which have an objective existence in nature.

Professor Mivart considers that such typical conceptions are copies of divine originals, and respond to prototypal ideas. Even if we regard organisms from all the points of view possible to us, we can but attain to a very imperfect conception of such organisms. The existence of these various homologies, serial, lateral, &c., renders it plain to any one who ponders over them, that there is in each individual animal a peculiar form or energy which actually results in the complex phenomenon above described. And just as species and genera do not exist as species and genera except mentally, and yet really exist objectively in those individual characteristics which furnish specific and generic characters, so the peculiar force referred to may be spoken of as that of the species, though of course it has no existence really except in the organic activities of the individuals which compose such species. To our minds the arguments of Prof. Mivart carry more weight than novelty can bring. They harmonize well with philosophical teaching, as well as with the highest anatomy. The postscript to Prof. Mivart's work is called for by an unannounced reproduction by Prof. Huxley of his criticism on the "Genesis of Species," of which treatise we regret to observe that he partly misapprehended and partly misrepresented the arguments. On the nature of the personal dispute which has taken place between Profs. Mivart and Huxley we shall say little. It is obvious that it is a mere question of facts, and not of inference. Prof. Mivart quotes the words which Prof. Huxley himself used some years ago. "The question has thus become one of personal veracity. For myself I will accept no other issue than this, grave as it is to the present controversy." That Prof. Huxley should not have been acquainted with such theological writers as Suarez, Pianciani, and Norris was a fact which if he had frankly confessed, no censure could have been attached to him for ignorance of works which a mere biologist does

not always peruse. But when such ignorance is combined with a fatal facility of accusation and covert hints of literary dishonesty on the part of his neighbours, most prudent men will recommend him study and silence. Above all, he must learn never again to read a work merely *ad hoc*. We regret that a mind which has been so industrious should have been induced either by the desire of approbation, or by the exercise of an aborted critical faculty, to call on himself the severe literary disgrace with which he is visited in the concluding chapter of the present work. For him, however, a bright future awaits; the peaceful perusal of the authors he has so glibly quoted; and the consideration that he was too hasty in bringing accusations which in calmer moments his naturally quick perception will perceive that it was impossible to justify, or even palliate. Prof. Mivart's work is written in a deeply scientific tone, and reflects that thorough knowledge of the scope and tendencies of modern biology which distinguishes the learned teacher of comparative anatomy at St. Mary's Hospital. Clear, readable, and profound, it will well rank with the most brilliant productions of Hæckel or Broca.

**A Manual of Family Medicine for India.**<sup>1</sup>—Considering the large European population of India, many members of which are far removed from medical assistance, it seems strange that until recently no work existed in India similar to the manuals of domestic medicine which have been long in use in this country. It was not until 1871 that the desirability of such a guide for the use of the officers in the Forest Department was brought by the officiating Inspector-General of Forests to the notice of the Government of India, who, recognising the necessity for such a work, offered a prize of Rs. 1000 for the best medical vade mecum suitable to the wants of the officers in the Customs, Opium, Forest, and other departments, whose duties necessitated them to reside frequently beyond the reach of medical aid.

The prize for the best essay, fulfilling the stipulated conditions, was awarded, in 1873, to Surgeon-Major W. J. Moore, of the Indian army, well known from his writings on Indian diseases, and has been published under the authority of the Government of India. Medicine chests, containing the medicines and instruments recommended for use in the work, are prepared by the General Apothecaries' Company.

The book is a pretty large manual of its class, clearly printed in large type, and illustrated by fifty-seven wood engravings.

It is divided into five chapters, the first of which contains a list of medicines, sixty-seven in number, proposed for the medicine

<sup>1</sup> *A Manual of Family Medicine for India*. By Surgeon-Major W. J. MOORE Bombay Army. London, 1874.

chest, and a short graphic account of the appearances, properties, principal uses, and doses of each are given.

The number of medicines seems to us large, and some of them decidedly dangerous (for example, chloroform), to be dealt with by non-professional persons; but the author thinks that it is not excessive, considering that there are 2700 medicinal preparations in the 'British Pharmacopœia' of 1874. However, if attention be paid to the directions given for their administration mistakes will hardly occur.

The chapter on diseases forms the principal part of the volume. Almost every malady that human flesh is heir to is treated of shortly and plainly, so as to be understood and managed by the non-professional reader in the absence of medical aid.

Directions for the diagnosis of diseases of the chest and liver, cholera, fevers, &c., in other words, the chief Indian diseases, are given in detail; and though a great many technical terms are used, they are always explained.

Ague being the most common, and often the most intractable affection, and quinine the recognised remedy, the different modes of its administration might have been stated with advantage.

Nothing is said about the value of hypodermic injection of a solution of the neutral sulphate of quinine, in some cases in which large doses given by the mouth produce no effect on the fever; nor is the reader told that it may be administered with beef-tea, &c., by the rectum.

Testimony, however, is borne to the efficacy of Warburgh's tincture (the principal constituent of which is quinine) when other medicines have failed, and full directions are given to ensure its effects when taken.

Accidents and injuries of every conceivable kind are described in as plain language as possible, and full directions are given for their treatment, and with such clearness, that this part alone might form a complete surgeon's vade mecum, not only for the non-professional, for whom it is designed, but for the young professional also. Amongst other injuries, snake-bites, bear and tiger-bites, are described, and the best treatment pointed out, and with reference to snake-bites it is worthy of notice and remembrance, that if the marks of the bite show more than two points the probability is that the snake was not of the poisonous variety. It was very just and proper to mention mosquito-bites, as they cause endless annoyance; but in describing flea-bites and their appropriate treatment, the author shows that he verily despiseth not little things.

In every case of serious accident, or where operative interference attended with danger is rendered necessary, medical assistance is properly recommended to be procured, and it must be said that the surgical directions, in the absence of professional aid, are plainly

and intelligibly written, and will form a good guide under the circumstances. A non-professional person, however, will hardly undertake to use the stomach-pump or to pass a probang on a patient without some previous experience, neither will he be inclined to perform any severe surgical operation.

The directions for the passing of a catheter when persons cannot make water, from various causes, cannot be too precise, and might have been supplemented by stating the average length of the urethra in the male and female, as well as its course.

It should also have been stated that if urine does not flow after the catheter has been passed for eight or nine inches, it is to be withdrawn a little, and then if urine is still absent the chances are that the eyes of the instrument have become blocked, and require to be cleaned.

The easiest form of elastic catheter for a non-professional to pass, without the stilet, is the French—the *catheter à boule*; but if they be ordinary gum elastic catheters, which are intended for the medicine chest, they should be periodically replaced, as they soon become deteriorated in various ways by the effects of heat, and consequently might break off in the bladder—a not unusual occurrence. It might also have been recommended that previous to attempting to pass a catheter by a non-professional the effects of hot fomentations to the lower part of the belly and hip baths should be tried.

Large numbers of prescriptions for various diseases are given, and a great deal of special information for nursing the sick; how to make the various kinds of hot, cooling, and soothing applications; the various kinds of baths; directions for dry cupping; how to apply leeches; and the nature and use of disinfectants.

The fourth chapter is devoted to an account of the best means of preserving health in India, the description of the climate, and how the latter is influenced by the geological and physical formation of the country, such as mountains, sand-tracts, rivers, marshes, &c.

There is one point, with reference to the curative effects of hill climates, that cannot be passed without some remarks, for at page 455 he states—"The very common error, however, of expecting Indian hill climates to cure disease should not be entertained . . . . As a rule, it is only those cases of ill-health when no specific disease exists which are benefited by change to the hills."

In reply to this we know from the 'Army Medical Reports' that the actual experience of medical officers who have served in the hills with large numbers of sick, suffering from all manner of organic diseases, is quite opposed to the author's view; and, indeed, we are of opinion that the time is not far distant when the curative effects of the hill climates of India, especially the Himalayas, in all curable diseases, will be more generally recognised.

It is quite impossible to discuss, in a limited space, the various



topics referred to in this manual; but there is a paragraph under the heading *Malaria*, page 456, which attracts our attention, and it runs thus:—"It (malaria) is also more potent near the surface of the ground than at any elevation, and its progress generally with the wind, is stayed by trees, by water, and by *artificial screens as gauze netting*." It is a very remarkable thing that, regarding the nature of some diseases, we have made no progress during the last fifty years, though investigators have not been wanting; but when we are told that malaria, which has the credit of causing more than one half of the diseases of the tropics, is stayed and its progress prevented by artificial screens as gauze netting, we will suppose on the principle of Davy's safety lamp, it must be admitted that some advance has been made towards a solution of its real nature. We wonder that this discovery has not been made known all over the world, for nothing would be easier than to apply a netting of any size over the face, or whole body if necessary!

The fifth and last chapter treats of the feeding and management of infants—subjects of the greatest importance when it is considered that the mortality of European children alone, chiefly from disease of the digestive organs, is more than 140 per 1000.

Altogether the manual cannot be too highly spoken of, and had not the author earnestly entered into the compilation of the work for love of it and a desire to benefit others, we are sure the Government prize of £100 would not have been a sufficient stimulus to do so.

If the book has not been already translated into the principal Indian languages it should be done at once, for we can imagine no greater boon to the Indian public, especially those living in villages far removed from proper medical assistance, than a work of this sort.

**Health in India for British Women.**<sup>1</sup>—If it were desirable to show the increasing importance of our colonial possessions, and in particular our great Indian empire, we might point to the number and character of the works which have of late years issued from the press, either in this country or abroad.

In a class by themselves, however, must be included those lengthy annual official sanitary and statistical reports, the compilation of which entails immense labour, but possibly labour only commensurate with the importance of the subjects treated of, and the great extent of country over which they profess to deal. We have also from time to time works of a more special character, some of which are intended to show us how we are to add to the necessities, comforts, or luxuries of life in the tropics.

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<sup>1</sup> *Health in India for British Women and on the Prevention of Disease in Tropical Climates.* By E. J. TILT, M.D. London, 1875.

For years past the intercourse between this country and India has been steadily increasing, and now, with the greater facilities afforded by the Overland route and the Suez Canal, the journey is deprived of many of its former inconveniences, and one may venture on such an expedition, especially in winter, with a feeling of comparative ease, and with every chance of deriving a great deal of instruction and pleasure.

There is in India a very considerable permanent European population, especially in the hill stations, as at Simla, Mussooree, Nyne Tal, &c., in Bengal, as well as in the Bombay and Madras presidencies, independent of the large number of European troops scattered all over the country.

When Europeans in the plains of India suffer from any long continued climatic disease, functional or organic, they often find it more convenient and cheaper to return to England for a change, rather than try the effects of some of the magnificent hill climates of India; consequently large numbers of invalids, especially ladies, connected with the civil service and the various public departments, arrive in this country from India, China, &c., at all seasons, and naturally seek advice, according to the nature of their several affections, from some famous metropolitan specialists, such as Drs. Tilt, Cobbold, &c.

Dr. Tilt has been engaged during the last twenty-five years in the treatment of patients suffering from uterine diseases contracted in the East and West Indies, China, &c., and, we presume, partly as the result of that experience, he has published this small volume, entitled '*Health in India for British Women, and on the Prevention of Diseases in Tropical Climates.*'

We are not quite sure it is advisable for any medical man to lay down rules for the guidance of health or the prevention of disease in a climate in which he has never practised, or in which he has not lived—at all events sufficiently long to become acquainted with its peculiarities; for without such experience he cannot be considered to speak with authority, and he must trust to the works of others or mere reports for his information, which cannot always be relied on.

That this little work, however, has been well received by the public there can be no doubt, as it has in a comparatively short space of time passed through four editions; and, indeed, no one conversant with the mortality and the diseases of women and children in India will deny that such a work, if useful, and from whatever source, was uncalled for.

The author, after a short introduction, deals with the climate of India, Indian pathology, the diseases of European women in India, their treatment and prevention, and, lastly, the colonisation of India and Indian sanitation.

He states that "the object of this little work is to inquire into all that relates to our countrywomen in India;" but he might have added our countrymen also, as the rules he has laid down in many cases are very general.

He commences his labours by a rough sketch of the geological formation and general configuration of Hindostan, and it is explained how the proximity of the mountains, forests, lakes, and seas, influence the rainfall, which varies in different places from a few inches to several hundred, thus producing a variety of climates. The cold, hot, and rainy seasons of the year are experienced more or less throughout India with almost unerring periodicity, and hence the author facetiously remarks that "the climate of India may be figured as a succession of annual ague fits, for every successive year brings its recurring stages of cold, of hot, and of sweating weather."

Practically this division of the climate is unfortunately not well marked as regards the second and third stages, which go hand in hand. Nevertheless, in many parts of Scinde, which is considered one of the hottest places in India, there is in the hot season a cool breeze at night, which enables punkahs to be dispensed with, and consequently refreshing sleep can usually be obtained.

In a large country like India, as might be expected, the temperature varies considerably in different places, and it is surprising to what extent vegetation and trees influence it. It strikes us forcibly that the climate in many parts of India could be much improved by preserving a healthy vegetation and by planting judiciously selected trees in unhealthy localities, as has recently been done in many military stations. We have heard that at one period large forests existed in the Punjaub, but in the course of successive wars they were cut down, with great detriment to the climate.

Human nature can bear, after a little practice, a very great amount of dry heat, but when much moisture is added it becomes very trying. At Calcutta, which in the hot season possesses a moist climate, the mean temperature is stated to be  $83.3^{\circ}$ , while that of the high table-land of the Deccan is  $82.6^{\circ}$ . At some stations, probably owing to local conditions, the temperature rises to an enormous height in the hot season, and as an example of this high temperature it is stated that at Saugor "during the summer of 1864 the temperature inside the house was  $110^{\circ}$ , although light was excluded and the tattiées and thermantidotes were brought into play." It is to be hoped such an occurrence is extremely rare, if not the death-rate must be high at Saugor, which is a large station, containing many European and native troops. One might naturally venture to ask, Was the thermometer under the circumstances quite trustworthy?

The Punjaub is said to be the healthiest province, and that there

diseases assume a milder form, and that the mortality is lower than in any other part of India; but it is purely theoretical to state that cholera and other diseases are influenced by sandy and alluvial soils and stagnant water.

Hill climates are not so well spoken of as they deserve, for at page 17 it is stated that "all medical authorities agree that hill stations in India only serve the purpose of Brighton or Scarborough in England, that during the hot season they are admirably calculated to improve the health of those only debilitated by heat, but that they are of no use to those who have structural disease of any internal organ—diseases which are greatly aggravated by remaining in the hills during the rainy season, when haze or mist, clouds or rain, are very frequent."

The evidence brought before the Sanitary Commission of 1860 as to the state of the army in India was not quite conclusive on this point, and some of it, perhaps, supports the above views; but we may reasonably inquire on what was that evidence based? Was it based on a careful observance and a minute examination of facts, or was it based only on theory or imperfect knowledge? It is plain, then, if it were based on either of the latter, such evidence cannot be received. The beneficial effects of the hill climates of India, in almost all cases of disease, both functional and organic, that are susceptible of improvement or cure in any climate, are yearly becoming more apparent and more widely known, notwithstanding the assertions of some reputed Indian medical authorities. In proof of what we state, we beg to draw the attention of those who may take an interest in India and her hill climates to a careful record of observations of their effects on the sick, compiled by Surgeon-Major Kellett, and published in the 'Army Medical Reports' for 1871, vol. xiii, page 219.

Dr. Kellett was in medical charge of Landour Convalescent Depot for two years, and had the treatment of large numbers of men suffering from all sorts of organic diseases contracted in the plains, the very worst cases being always selected for the hills; but unfortunately the period of residence averages only about six months—a period not sufficient, it must be admitted, to fully restore a debilitated frame, yet quite sufficient to enable an opinion to be formed of its effects on health and disease. That medical officer noted carefully the condition of the men on arrival, their respective weights, chest capacity, condition of their internal organs, &c., and again before departure from the hills, and found that most of them increased in weight, chest capacity, &c., and that the condition of the internal organs was improved, and that those men who lost in weight, &c., were men suffering from fatal diseases, incurable in any climate.

It was found that men suffering from ague on arrival were

speedily cured, and that cases complicated with syphilis or hepatic affections were much improved, but the most marked salutary effects of the climate were observed in phthisical patients, who, it is stated, "almost invariably improve, and often recover," provided the lung-destruction has not gone too far.

Another medical officer, who has had the advantage of the medical charge of the large sanitarium of Kussowlie in 1871, gives further proof, and in the work just quoted, page 217, he writes "that Indian hill climates are eminently curative in nearly all Indian diseases, as they are by far more genial and equable than any English military station;" and again, "that invalids who die at an Indian sanitarium are generally men who could not recover in any climate."

Another objection, viz. hill diarrhœa, has been urged against hill climates; but only some persons suffer from it, and those who do suffer most probably do not guard against the change in temperature by providing themselves with sufficient and suitable warm clothing.

Dr. Tilt states (page 21) that the factors of a tropical climate are HEAT, COLD, DAMP, and MALARIA, and that as we approach the equator "vegetation increases in variety and luxuriance, animal life is exhibited after more and more numerous, larger and more beautiful patterns," and that "Man participates in the universal law that associates life with heat," adducing, in proof of this, that the two most numerous families of the human race, the Hindoo and the Chinese, have from time immemorial flourished on each side of the Himalayan range.

It might be granted that, as regards proliferation, the inhabitants of the tropics may compete successfully with the plants, but as regards physique and stamina, if Dr. Tilt had lived even a very short time in the regions of great heat he would have seen that there is no analogy. Look at the weak, effeminate Hindoo, or the puny, debilitated inhabitants of lower Bengal and many parts of India!

The author states nothing new regarding the nature of malaria that fertile subject for speculation, more than that "it is most virulent as the equator is approached, where it helps to check a too abundant population," assertions which are questionable; but what will our readers say when they find that he talks of dry heat as a tonic! We think that one hot season in the plains of India would induce him to change his mind on that subject, and we have no doubt but that our Indian friends will agree with us.

The chapter on Indian pathology is a short but interesting one, and treats, amongst other matters, of a condition of the system called spanæmia (or scarcity of blood), which it appears is somewhat different from the condition generally known as anemia, but we fail to see the difference. Spanæmia may affect Europeans or natives,

and is the invariable accompaniment and sequence of malarious poisoning; and we are told, "if placed under similar conditions, spanæmia is more marked in women than in men; it depends in differences in the texture of her tissues and in the composition of her blood, as compared with those of man—differences that render her so liable to some form or other of spanæmia, even in England, where it is seldom observed in man." Is, then, the normal composition of woman's blood different from that of man's, and, if so, in what does this difference consist? Women in India suffer from the same diseases as men do, and there is no peculiarity in the disease of the former that cannot be explained by the difference of sex.

European women especially, as well as the native women, suffer much from diseases of the generative organs, and, on the authority of Dr. Duncan, it is a common occurrence for women to be brought to the Calcutta General Hospital suffering from dysentery caused by inflammation of the womb. The diseases of women and children in India have not, from some cause or other, claimed the attention which they deserve, though the rate of mortality and sickness is, and has always been, very high; and, according to Dr. de Renzy, the former amongst soldiers wives varies from 25 to 54 per 1000 annually.

This is a high death-rate, certainly; but it is not stated what it is owing to, whether the result of ovario-uterine or other diseases, though Dr. Stewart says "that eight out of ten of the European female residents are habitually subject to deranged menstruation, leucorrhœa, or to cervical inflammation." Such a condition of the European female population, vouched for by the late professor of midwifery in the Medical College, Calcutta, and physician to the Hospital for Native Women, is truly alarming, and any measures recommended for the relief and prevention of such an amount of suffering should have a fair trial.

That the mode of life and the condition in life may have something to do with this amount of sickness, independent of the effects of climate, are worth consideration; for we have found that the wives of soldiers were less affected with uterine diseases than those of officers. Good sound advice, the result of the author's own experience and that of others, is given for the prevention of some of these diseases, and for enjoying good health in India; how a woman is to conduct herself on arrival; the quality and quantity of clothing she is to wear; what she is to eat, drink, and avoid; and the amount and nature of the exercise she is to take; in short, her personal hygiene,—are fairly considered.

The author considers it wrong for women to go to India until they are of age, unless they have been born there, and that women who suffer from chlorosis, biliary derangement, morbid menstru-

ation, or uterine disease in a temperate climate, should not go to India.

Generally speaking, he is no doubt quite correct; but, strange as it may appear, there are many women who suffer much in this country benefited by the climate of India, and some who have been barren here have offspring there.

The book concludes with a chapter on the colonisation of India and on Indian sanitation—subjects of vital interest.

With reference to the colonisation, the popular impression is, and we believe has been hitherto, that the offspring of pure Europeans would die out in three or four generations.

We can hardly believe that such would really happen as regards Europeans living in the hill climates at all events, and further, more evidence is wanted to prove that they would die out in the plains, in which there are many healthy stations.

In what part of the world do we find healthier or more robust children than those in the schools of Simla and Mussoorie and other hill climates? Observe the hill tribes of the Himalayas, badly clad and badly fed; they suffer but little from disease, and, physically, few are their equals and less their superiors, and therefore it is a matter for congratulation that the Indian Government is taking steps to increase the number of troops in the hills, also to form European colonies there, and thus lessen still more the sickness and mortality in India.

To have brought about this great diminution in disease and death many causes have been in operation besides increased sanitation, and to the medical department, to which the soldier owes so much, and to Lord Napier of Magdala, the British and Indian armies should be deeply indebted. Of late years amusements, trades, gymnasia, exercises of various sorts, camps of instruction, &c., and last, but not least, temperance societies have been introduced in regiments serving in India, with the very best results, and in proof of this the Commander-in-Chief has recently stated that the "offences of the temperance men compared with those of non-abstainers are as one to forty, nearly;" and therefore it may be reasonably inferred that the same causes have operated in the diminution of preventible diseases, many of which are caused directly or indirectly by intemperance.

In this short notice of a little book on a great subject, many points of interest have been passed over or touched on very lightly, and though the work is partly compiled from the labours and experiences of others, we nevertheless heartily recommend it to those interested in the welfare of British men, women, and children in India, and have no doubt but that its perusal will give them pleasure and profit, as it has given us. It will be especially useful to the Indian medical practitioner.

**Roy on Burdwan Fever.**<sup>1</sup>—In our last volume but one we noticed the essay of Dr. Roy on the fever which has prevailed for some years past in the district of Burdwan, in Lower Bengal. The present work is a reprint of that essay, with the addition of a short section on malaria, in which the author alludes to the opinions that were under discussion some months ago, as to the non-existence of the febrific miasm so called, and the dependence of the fevers usually attributed to malaria on simple chill, and shows that the occurrences at Burdwan were opposed to the latter view. In an appendix there are some remarks on the past and present state of medicine in India, and observations on the solvent action of papaya juice on nitrogenous articles of food. There is a belief, widespread in the tropics, that the papaya or papaw plant has great influence in making meat tender, or even in hastening its putrefaction. The author says—"It is the practice amongst native cooks in India to add a few drops of the milky juice of the plant under consideration to tough, old meat, to make it tender and supple. Four years ago, anxious to ascertain whether any such virtue really existed in the plant, I added a few drops to a pound of minced goat's meat, and stewed it on a slow fire. To my surprise the whole ran into a diffuent mass in five minutes, owing to a larger quantity of the juice having been used on the occasion than was necessary to make it tender and eatable" (p. 157). The author brought some of the desiccated juice (prepared by scarifying the unripe fruit and drying the fluid exuded in the sun) with him to England; portions of this dissolved in water showed considerable solvent powers on meat, hard boiled white of egg, and gluten. He says "the whole action so resembles healthy digestion, that I wonder we have not availed ourselves of this medicinal property in cases of invalids and dyspeptics, to substitute a process of artificial digestion," and expresses his intention of renewing the investigation of the subject as soon as the pressure of his duties at Burdwan will permit.

**Rumsey's Essays on Medical Statistics.**<sup>2</sup>—The author of this treatise and his labours in the cause of sanitary medicine are so well known and appreciated both by the Profession and the public, that it is enough to announce a new volume from his pen to secure for it due consideration. To those acquainted with the character of the sad affliction that suddenly befel him, it was pleasing to obtain, by the publication of the present treatise, evidence of recovered ability for intellectual and literary work; and we hope to receive future fruit of his well instructed mind in connexion with sanitary science and statistics.

<sup>1</sup> *The Causes, Symptoms, and Treatment of Burdwan Fever, or the Epidemic Fever of Lower Bengal.* By GOPAUL CHUNDER ROY, M.D. New Edition, London, 1876, p. 168.

<sup>2</sup> *Essays and Papers on some Fallacies of Statistics concerning Life and Death, Health and Disease, with Suggestions towards an Improved System of Registration.* By HENRY W. RUMSEY, M.D., F.R.S. London, 1875.



The volume now submitted to our notice is essentially a collection of papers or memoirs Dr. Rumsey has, from time to time, contributed to one or other scientific society or medical periodical, and represents the author's criticisms and convictions regarding some of the principal questions of sanitary administrations, dealt with from a statistical point of view. The author has made it his special business to point out the shortcomings and fallacies of official statistics and of the method employed in collecting them, and to indicate the direction in which truth is to be discovered and the modes of inquiry be rendered more accurate and precise.

Those who have been concerned in statistical inquiries respecting the diseases and mortality of the whole population, or of the inhabitants of a limited area, will thoroughly apprehend the value of Dr. Rumsey's remarks on the influence of numerous social and other conditions in modifying results represented by crude figures; and those who would embark on such investigations will do well to read and duly weigh those remarks before committing themselves to inferences appearing at first sight inevitable. How much error, indeed, is spread abroad by unsound statistics, praiseworthy for accuracy and minuteness from the mathematical point of view, valueless and deceptive when submitted to the logic of facts! As a safeguard to such fallacious statistical deductions this volume will prove a corrective.

The essays contained in "Part first" have chiefly an historical interest in sanitary legislation. They discuss matters which subsequent legislation has more or less completely dealt with; but the convictions and arguments advanced by the author afford proof of his sagacity and fore-sight by the very reason that they subsequently secured public acceptance. It is in "Part second" that Dr. Rumsey enters upon a critical survey of vital and sanitary statistics, remarking on the nosology of disease adopted for the purposes of registration of mortality, on the defects attendant on returns of deaths, or the calculation of the death-rate in the population, and the value to be assigned to that rate as an indication of the hygienic condition of a district. In an appendix he discusses the provisions necessary to the registration of disease, and presents a verbatim report of the important evidence he gave before the Royal Sanitary Commission. Those who have been interested in sanitary work and legislation and have followed up their gradual outgrowth into matters of state importance from their very humble beginnings twenty or thirty years ago, will be familiar with the views and arguments of Dr. Rumsey, now a veteran in the cause. And even when they cannot agree with him they will feel great respect for his opinions, which have possessed the merit of having always been fearlessly and honestly advanced. Lastly, by all such individuals this last contribution from his pen will be highly valued.

**United States Medical Library.**<sup>1</sup>—The publication of this fasciculus represents the inauguration of an undertaking of vast magnitude and of the greatest utility, viz. the formation of a National Library for the United States. It has been started under the auspices of the Surgeon-General of the United States Army, J. K. Barnes, but the active agent in its formation is Assistant-Surgeon J. S. Billings. By the energy and unflagging labours of the latter gentleman a comparatively small and little known library at the Surgeon-General's office has rapidly developed into a magnificent collection of medical works of all nations, well deserving the recently assumed title of "The National Medical Library," though essentially the medical section of the Library of Congress.

The number of volumes is 40,000, and there are almost the same number of single pamphlets. And it has been a leading object with Dr. Billings to collect periodicals and pamphlets, as especially rich in clinical records and original observations, and further as difficult of access to individual medical men.

From the introductory remarks it would appear that this library is a truly free one, and that it is used by practitioners at remote distances, who consult it either in person or by letter. Having, therefore, this free character and extended purpose, it became very essential to inform the profession at large of its contents. Hence the undertaking of the elaborate catalogue, of which the fasciculus before us is a specimen. To give an idea of the plan pursued and its completeness we will cite the subject abortion, as catalogued. The titles of the works upon it occupy six royal octavo pages, each page of two columns; the titles of pamphlets and articles in journals being printed in very small but clear type. The works are severally arranged under their authors' names, with their full titles, date, and place of publication. So with papers and cases appearing in medical journals, we find the name of each author, that of the journal in which the paper is published, the date of publication, the page, and title of the essay. Nothing could be more complete or more easy of reference. We should add that the works relating to abortion are disposed in two groups—the one, including treatises on the condition in general, the other, those on the jurisprudence of abortion.

When completed, on the plan now submitted to notice, the entire catalogue will form five volumes of about one thousand pages each.

We heartily wish Dr. Billings health and success in carrying

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<sup>1</sup> *Specimen Fasciculus of a Catalogue of the National Medical Library.* Under the direction of the Surgeon-General, United States Army, Washington.

out this vast amount of labour in the interests of his professional colleagues; and we feel we can do him no better service than by calling the attention of our readers to his desire to further enrich his library both by independent treatises and by the more ephemeral literature of the present and past days, to his readiness to receive donations of works, and to his courtesy in acknowledging them.

**Blyth's Dictionary of Public Health.**<sup>1</sup>—This is a very comprehensive treatise, dealing with all matters connected with sanitary and state medicine; and at this day, when sanitation is professedly a chief object of legislation, and at the same time a most popular subject, it is just the sort of book of reference that might fairly be looked upon as much needed. At the same time an examination of its contents satisfies us that it is a book that should be highly appreciated. The quantity and varied character of the matter it contains is enormous. Its nearly 700 pages are printed in double columns and in rather small type. Interspersed in the text are a few illustrations, and there is besides a large coloured map exhibiting the geographical distribution of disease.

The plan of the work as proposed was that of Tardieu's Dictionary of Hygiene, but the author found it necessary to greatly modify it, and largely to strike out a plan for himself. We consider, however, that he might have still farther departed from the French model with advantage, and we would have had him exclude the many paragraphs devoted to ordinary pharmaceutical preparations, which tell the reader nothing but what may be found in any treatise on *Materia Medica*, and what, indeed, has but little to do with public health. Our desire is, however, not to find fault where so much good matter exists, but to recommend the volume to our readers. We hope that a call for a new edition will enable the author to correct some errors we notice in articles on sanitary measures, as for instance some relating to the Factory Laws.

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<sup>1</sup> *A Dictionary of Public Health, comprising Sanitary Chemistry, Engineering, and Legislation, the Dietetic Value of Foods and the Detection of Adulterations.* By ALEXANDER WYNTER BLYTH, M.R.C.S., &c. London, 1876.

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## Original Communications.

**Notes on Syphilis in the Insane.**—By W. JULIUS MICKLE, M.D.,  
Medical Superintendent, Grove Hall Asylum, London.

(Continued from p. 195.)

### PART II.

*A.—Résumé of cases of insanity coexisting with syphilis, with reference to the so-called "varieties of syphilitic insanity."*

*B.—Acute forms of insanity intercurrent in secondary syphilis.*

*A.*—In offering a summary of the principal cases of insanity with syphilis occurring in the practice of this asylum it is necessary, in the first place, to recapitulate those detailed in Part I.<sup>1</sup> That portion of my contribution was devoted to the recital of ten cases of *intra-cranial* syphilis with insanity. Concisely stated, their leading features are these, viz :

In CASE 1.—Dementia, headache, epileptic seizures, transitory hemiplegia, recurrence of epileptic attacks assuming an excessively violent form and producing death. Autopsy : syphilitic disease of brain, of cerebral arteries, and of viscera.

In 2.—Sudden paresis of articulation and deglutition and incomplete hemiplegia, followed by intellectual and moral deterioration ; temporary attacks of inability to swallow, and of vaso-motor disturbance. Subsequently, dementia, persistent incomplete hemiplegia, impaired articulation, speechlessness, intestinal torpor, and contraction of limbs. Autopsy : syphilitic disease of cerebral arteries, corpus striatum, and viscera.

In 3.—Dementia ; exceedingly feeble cardiac action, circulation, and peripheral nutrition ; dyspnoea, pallor, &c. Autopsy : syphilitic disease of heart and of cerebral cortex and meninges.

In 4.—Intense headache, dementia, and symptoms simulating general paralysis of the insane, extreme general motor paresis, incomplete hemiplegia, symptomatic paralysis agitans, disturbed cardiac innervation, and difficulty of deglutition. Recovery.

In 5.—Intense cranial pain for a prolonged period, epileptiform

<sup>1</sup> 'Brit. and For. Med.-Chir. Rev.,' July, 1876.

convulsions, and transient paralysis. Subsequently; dementia, hemiplegia of considerable duration, blindness from the effects of extreme double optic neuritis, incomplete and partial unilateral anæsthesia (tactile), partial deafness, constipation, epigastric pain, recurring epileptiform attacks. Recovery from most of symptoms.

In 6.—Chronic insanity with delusions and vivid hallucinations leading to excitement, violence, &c.; with insomnia, violent headaches, and osteocopic pains, pericranial node, nausea, vomiting, impaired sight, and early optic neuritis. History of severe secondary syphilis, of rash, node, orchitis, &c.

In 7.—Syphilis; temporary paralysis of third cranial nerve, persistent hemiplegia, intellectual and emotional weakness, and, finally, confirmed dementia: syphilitic periostitis.

In 8.—Chronic mania, violent, restless, excited, noisy, delusions of injury, vivid hallucinations, corporeal illusions; later on, absurd exalted delusions as to personal identity, temporary paralysis of sixth cranial nerve. Syphilitic lesions of skin, periosteum, and cerebral nerve.

In 9.—Fits, hemiplegia and impairment of speech; improvement; severe cranial pains, sudden intellectual derangement passing into a condition of mental weakness, with maniacal symptoms, with delusions and hallucinations of sight and hearing, and, latterly, into more confirmed dementia. General motor impairment and feebleness. History and symptoms of syphilis.

In 10.—Syphilis. Intellectual and moral alienation and weakness, hallucinations of sight, motor paresis and inco-ordination; attack, in which the gait became ataxic, with impaired sight and with temporary unilateral paralysis of third cranial nerve. Complicated by effects of intemperance. Recovery.

Three clinical features are worthy of notice in these, and similar, cases.

The first is the marked tendency to mental impairment, failure, or dilapidation; the predominance of negative intellectual symptoms, often associated with failure, weakness, or loss of the moral sense, of the self-regarding virtues, and the general inclination to a degraded state of feeling as evidenced by the habits of the patients. In some, early or intercurrent outbreaks of maniacal symptoms occurred; in a few there was causeless depression or fear; or emotional weakness, as displayed either in a lachrymose tendency or in a disposition to uncalled for merriment.

The second broad clinical aspect of importance in the above cases is that almost invariably there were motor symptoms of the paralytic or of the convulsive type. In the former—the paralytic—type, motor failure was in the form of hemiplegia, or palsies of cranial nerves, or impairment of articulation, or motor paresis and incoordination of a wide-spread kind, or impaired inner-

vation of visceral organs. The latter—the convulsive—type of symptom showed itself more particularly in epileptiform attacks, hemispasm, general convulsive seizures, tremors, or symptomatic paralysis agitans.

The third general feature is that sensory symptoms were frequent. Of these the most striking was intense nocturnal pain, especially cranial pain; while anæsthesia was far less frequent. Nor did the special senses escape, for impairment of sight, blindness, or unilateral deafness were occasionally found. Rarely were hallucinations of the senses observed.

These then were the other symptoms commonly associated with the mental weakness, impairment, dementia. In only one or two cases was there a decided development of insanity partaking of the features of ordinary forms of idiopathic insanity. Even where *intracranial* syphilis is found in the insane it is only by a close and accurate study of each case that it can be decided as to whether the constitutional syphilis is the cause of the mental symptoms; or modifies, without having the first place originated, them; or, on the other hand, is merely a chance accompaniment. The evolution of syphilis often being a long process, other causes may in the meanwhile act upon the person who has contracted syphilis, and may be efficient in the production of insanity. Therefore it is that the appearance of syphilitic lesions before the inception, or during the course, of mental disorder or decay, does not justify the rash conclusion that the specific disease is the basis on which the mental affection rests. In nothing, often, is there more doubt than as to the influence or group of influences which has broken down the mental edifice, whether its original materials and the plan of its construction were sound, or whether from the first foundation the structural constitution of the brain was defective and its functional energy ill-balanced and not aptly fitted to resist the strain of the exigencies or crises of life.

When, therefore, cases differing extremely in their histories and clinical features are grouped together under the name of “syphilitic insanity,” and nosological varieties of this are described, it is fitting to inquire upon what bases the descriptions of these varieties really rest. That a patient may contract syphilis, and that insanity may occur some weeks, months, or years afterwards, surely tells us nothing as to the etiology of the mental affection. It is stated that syphilis may originate insanity at periods of its duration varying from a few weeks to many years; that in the early stages mental disorder may be induced by the anæmia and blood changes of syphilis, and, later on, by inflammations or congestions originated by syphilis, or, again, by syphilitic neoplasms either affecting or invading the brain from without, or developing within it. I cannot but think that many of the cases described as of the first and

second of these kinds were in reality due to other causes than syphilis. Where actual syphilitic lesions of the brain and its membranes occur the cases have a certain family resemblance,—greatly varied as the symptoms are by the situation and extent of the lesions, and by other causes. But where no gummatous or other lesion of the tertiary stage affects the encephalon or its tunics, and yet there is insanity with antecedent syphilis, the cases stand in a different category—so different as to render the application of the term “syphilitic insanity” inappropriate to them, or almost so, for we must admit that the instances in which such lesions do occur and do produce mental disorder or decay are the natural elements of the group to which that term is applicable, (if we use it). Not that the psychical disorder is usually caused directly by the syphilitic lesion, but by common nonsyphilitic pathological changes set up secondarily by it. When, however, we pass from the cases in which organic naked-eye syphilitic disease affects the brain and its membranes, we pass from a region of some certitude to one of doubt and obscurity. Where no intracranial syphilitic processes are developed the origin of insanity can rarely be attributed to syphilis with scientific accuracy. In a series of such insane and syphilitic patients the clinical features and general course of the mental disorder seem to be the same as those in an equal number of their non-syphilitic fellow patients, when the other circumstances and influences have been alike. That there is good ground for this assertion will be evident from an examination of the adjoining tabular summary of a number of instances amongst the patients here in which insanity and syphilis coexisted, but in which there was no evidence of organic intracranial syphilitic lesions such as existed in the cases detailed in Part I. In the tabulated cases now referred to, insanity of course occurred at a period subsequent to the syphilitic infection. All that is necessary for the present purpose will be made clear by placing the leading facts relating to the cases in this concise and tabular form, which can be referred to without interrupting the thread of the present discussion, and by which the recital of a long series of cases is avoided. (See table.) Besides these, there were many other cases with more or less evidence of syphilis, but without anything special in their general features.

On examining the summary of the cases in the table a great variety of symptomatological forms of mental disease are found supervening in syphilitic persons. There is no evidence of characteristic clinical features in them—nothing that marks them off from the ordinary run of cases—and the connection between their syphilis and their insanity seems, for the most part, to have been but a casual one. When, therefore, cases are described as examples of “syphilitic insanity,” in which the clinical features are the same as those in ordinary insanity, where at death no syphilitic lesions are found in

or about the nervous centres, and where the only evidence suggestive of syphilis as a cause is an infection occurring previously to the onset of insanity, it is evident that some more stringent proof of a causal relationship must be exacted before such cases can be admitted into that category. But for the moment, admitting the assigned etiology as correct, surely it is misleading to include under the same designation a case in which a gummatous mass on the surface or in the substance of the brain induces mental symptoms, with another in which these arise from the circulation of syphilised blood or from syphilitic anæmia. Surely the term "syphilitic insanity" becomes too elastic under these circumstances. To remedy the anomaly mentioned it has been proposed<sup>1</sup> to apply the term "syphilomatous insanity" to the instances of "syphilitic insanity" supposed to be due to syphiloma of brain. But if syphilomatous insanity, why not also gliomatous, myxomatous or sarcomatous insanity? and so on for every kind of brain tumour, during the continuance of which mental derangement or decay occurs.

It is convenient to notice here the varieties of so-called "syphilitic insanity." With this view it will suffice to examine the affection as described in the work of Wille,<sup>2</sup> and in the Morisonian lectures for 1873.<sup>3</sup> Wille describes the cases as most frequently exhibiting an early hypochondria, and then a progressive dementia, the course of which is sometimes broken by mental disturbances in the form of mania, melancholia, or acute delirium; in others, mania occurring first, and alternating with melancholia; in others, "persistent hypochondriacal melancholic ailments and delusions;" while in some the affection begins as acute delirium, or as monomania of grandeur; or, lastly, follows apoplectiform or epileptiform attacks. In many beginning acutely there was, later on, a tendency to dementia, and in a few to the symptoms of moral insanity. In discussing the early occurrence of syphilitic mental disease he quotes the observations of Leubuscher and others, to the effect that it may occur with the first secondary symptoms, or even preceding these. "This condition especially affects individuals whose brain (Griesinger) is organically troubled, who have previously presented symptoms of abnormal mental activity, or in whose families nervous diseases have frequently occurred."

Dr. Clouston, who wrote the section on "syphilitic insanity" in the Morisonian lectures, follows in much the same lines. The first variety of syphilitic insanity which he describes has the features we have previously mentioned as those of the mental and other derangements which often arise from gross intracranial syphilitic disease—

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<sup>1</sup> 'Journ. Ment. Sci.,' Jan. 1874, p. 555.

<sup>2</sup> Abstract by Dr. A. Addison, 'Journ. Ment. Sci.'

<sup>3</sup> 'Journ. Ment. Sci.,' April, 1875.



gummatous, inflammatory, or arterial. To this the title "syphilitic insanity" is applicable, if we use the term.

The second variety of the Morisonian lectures includes such cases as those just mentioned from Wille, where insanity of an acute maniacal or delirious form breaks out almost immediately or not long after primary syphilitic affection in a patient whose history is one of pre-existing disease of brain, or of previous attacks of insanity, or of hereditary neurosis. "Such cases are not so uniform as the others, sometimes the mania alternating with melancholia, sometimes disappearing for a time, the patient appearing to be quite well, or, lastly, assuming the type of ordinary idiopathic insanity. Dr. Hugh Grainger Stewart's cases were of this kind." It may be remarked, parenthetically, that there seems to be an error here as to the bearing of Dr. Stewart's cases. In his first case it is not stated that the patient had contracted syphilis at a date shortly anterior to the outbreak of insanity; it is merely asserted that he had suffered from syphilis. In the second case insanity was of one week's duration, and when admitted to the asylum the patient had a sore, apparently of a tertiary type, upon the leg. The syphilitic history of the third and last case is obscure.

It may well be inquired upon what proof rests the belief that the syphilitic virus induced insanity in this second form, as described in the Morisonian lectures? The cases appear, in all respects, to resemble ordinary acute forms of insanity arising in the young and middle aged, and often becoming chronic; and it would be matter of surprise if a few of these patients did not happen to have contracted syphilis just previously. What, then, marks them off from the similar ordinary cases in which the accident of syphilitic infection is not present? Is it the curability, or the amenability of the symptoms to specific treatment? Or is it the clinical aspect? Or, finally, is it the necroscopical record when death has subsequently taken place from intercurrent maladies? The distinguishing characteristic is not found in the first, for the recovery of some is a hopeless matter from the beginning. The second suggestion—that in the clinical aspect the differential distinction may be found—appears more hopeful, but it is to be feared that that hope is fallacious. The late Dr. H. G. Stewart, founding upon three cases, attempted to lay down certain characters as possessed in common by these cases when they had become chronic:—"1. They all occur in subjects who have suffered from syphilis. 2. The character of the delusions is similar. They all believe themselves the victims of conspiracy, persecution, and cruelty, undeserved. 3. They are all subjects of hallucinations of touch, hearing, and sight. 4. They are all suicidal, two of them having made actual attempts on their own lives. 5. They are all dangerous to others when under the influence of their delusions, and are quite unsafe to be at large. 6.

They are all worst at night, which circumstance may be dwelt upon as indicating the syphilitic nature of the disease. 7. They all suffered severely from cephalalgia. 8. The treatment produced but little effect, and it is feared they can only be looked upon as incurable . . . . .”

Here is an accurate description of clinical features which, however, are often found in insane non-syphilitic patients also. A considerable proportion of the soldiers under my care have delusions, hallucinations, &c., of the nature above described, without ever having had syphilis. The proportion of these is larger than in some other asylums, the insane population of which is mainly drawn from rural districts and agricultural or industrial pursuits. Not that all the cases referred to have presented every one of the seven or eight characters fully developed; nor, supposing the above quotation to be absolutely correct as the description of a syphilitic mental affection, would the majority of the patients suffering from it. Here, as elsewhere, the full typical development would be the exception. What has just been asserted above would, of course, not disprove the possibility of the blood change or nutritive impairment in syphilis being the cause, or an accessory cause, of mental aberration, but would go to show that the mental symptoms thus produced did not wear specific features.

Then, as to the third and last suggestion, no support is given by the necroscopical records to the etiology assumed for these cases. The description, I believe, has not yet been enriched by any post-mortem evidence. In one case, recently described,<sup>2</sup> there is no proof that the changes found at the autopsy were of syphilitic origin or that the insanity depended upon syphilis. If, as Mr. Hutchinson<sup>3</sup> suggests, “the tertiary growths occur in cell structures which have remained over in a quiescent state from the secondary period,” then, except where the mental outbreak is *very* early, we should expect, when insanity arises from the pathological processes of secondary syphilis and the patient happens to die later, to find gummatous regrowths in the nervous system developed in the lines laid down during the secondary stage. There is as yet no proof of such discovery under these circumstances.

That syphilis may *modify* the symptoms in the forms of chronic insanity just described is evident from clinical observation. The nocturnal pains of syphilis, its rheumatoid or neuralgic symptoms, may, like other depressing or wearing influences, aggravate the mental disorder, and especially at night. But this is totally different from an essential dependence upon syphilis of the mental symptoms in their origin or in their continuance; and, in short, the two affec-

<sup>1</sup> ‘Brit. Med. Journ.,’ vol. ii, 1870.

<sup>2</sup> ‘Journ. Ment. Sci.,’ 1875, p. 270.

<sup>3</sup> Discussion at the Pathological Society, London, 1876.

tions seem to be independent and only accidentally associated in most of the cases under discussion. The hæmic and other changes of syphilis in its early stages may now and then be the exciting cause of mental disease where the predisposition is strong, but for these cases the name "syphilitic insanity" seems undesirable.

The next variety of syphilitic insanity mentioned in the Morisonian lectures is that in which "it follows distinct syphilitic epilepsy or apoplectiform attacks, and in this way partakes of the character of ordinary epileptic insanity." Why, then, not *call* them cases of "epileptic insanity" (if Skae's classification be used), especially as "the instability of the grey matter," or whatever description may apply to the state of the nervous centres which gives rise to convulsion, is not a syphilitic change, but is always induced secondarily? Epileptiform attacks, indeed, often occur early in the course of the typical case of insanity dependent upon syphilis, where the mental symptoms do not flow from the symptom, epilepsy, but where the two orders of symptoms—the mental and the motor—are concomitant effects of the one specific lesion, and capable of appearing independently of each other. The fourth and last variety referred to in the lectures quoted is that in which the symptoms closely resemble those of general paresis. But as this is not finally claimed as a distinct variety of syphilitic mental disease it need not be further adverted to in this place.

Thus, then, of the four varieties referred to, only the first seems to maintain its place as a frequent and distinct form of disease. I think the occurrence of the second and third varieties to be far less frequent than has been asserted or implied in some quarters; that in the majority of the cases assigned to these varieties syphilis was not the real cause of insanity; and that those in which it was, are undeserving of being placed in the same pathological niche as the cases included under the first variety; but, rather, that they partake of the nature of insanity whose development in a predisposed person is assisted by the anæmia, toxæmia, or epilepsy of syphilis, and (adopting for the moment a classification analogous to Skae's) as coming more properly under descriptions such as anæmic, toxæmic, epileptic, and idiopathic insanity, than under the appellation of syphilitic insanity.

*B.*—This leads to a consideration of the acute forms of insanity intercurrent in secondary syphilis.

Considerable maniacal excitement, or even acute phrenzy, may occur in the course of mental decay or derangement produced by intracranial syphilitic lesions of the late stages. A good example of this is found among the cases by Dr. Reade,<sup>1</sup> of Belfast, and

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<sup>1</sup> 'Dublin Quar. Journ. of Med. Sci.,' vol. xiii, p. 53.

others in those mentioned by Dr. J. F. Duncan<sup>1</sup> and Dr. Wilks.<sup>2</sup>

In many cases such as these the outbursts of acute mental symptoms follow upon convulsive seizures caused by organic syphilitic disease of the tertiary type. But the cases now under consideration are those in which acute mental symptoms accompany the evolution of the secondary stage,—and especially its earlier periods—and in which the features of active mania, of delirium, or of melancholia are presented at the very onset, and, as a rule, quite independently of any epileptic or other nervous affection caused by syphilis. As just argued, it seems that the frequency of these cases has been exaggerated, that upon closer examination many appear to have had no actual dependence upon syphilis. In some of the few real cases the blood-change produced by the specific virus probably acts much as a dram of spirits acts upon the hereditarily weak brain in producing a temporary “mania a potu.” That early mental disease<sup>3</sup> may be produced is, moreover, rendered likely by the occasional, but rare, occurrence in the early secondary stage of syphilis of other nervous symptoms such as headache, insomnia, anæsthesia, neuralgia,—or hemiplegia and facial palsy.<sup>4</sup> I have no clear undoubted cases to offer of acute mental disease caused by syphilis in its earlier secondary stage. In the practice of this asylum I found several instances of acute insanity occurring at that period of the evolution of the specific malady. In one or two of these, the mental aberration had been attributed to the effects of syphilis by those who sent the patients here. It will suffice to give an abstract of the notes made on three of these cases, from which it will be seen that clear proof of the actual dependence upon syphilis is wanting, and that there is nothing special to distinguish these cases from acute attacks of insanity occurring in non-syphilitic patients under the same general influences and with the same general surroundings.

CASE 1.—The patient was an artilleryman, aged 22 years, who had had an attack of insanity three years before, and had been discharged recovered from this asylum after seven months treatment. After continuing well for some time he re-enlisted, and suffered from this second attack when he had completed fifteen months service. He contracted chancre for which he was placed under treatment. Four months afterwards he had an epileptic seizure,—his first one,—then symptoms of acute mania arose, then became sub-acute; excitement alternated with depression, he was suicidal at times, at others violent, and required constant surveillance. There

<sup>1</sup> ‘Dub. Quar. Journ. of Med. Sci.,’ vol. xxxv, p. 48.

<sup>2</sup> ‘Journ. Mental Sci.,’ 1874, p. 38.

<sup>3</sup> Cadell, ‘Journ. Ment. Sci.,’ Jan., 1874, p. 564. Zambaco, Obs., lxxxix.

<sup>4</sup> Bäumlér, ‘Ziemssen’s Cyc.,’ vol. iii, p. 220 (Trans). ‘Lancereaux,’ vol. ii. Zambaco, ‘Des Affections Nerveuses Syphilitiques,’ Obs. lxxiii. Fournier’s case.

was a strong propensity to mischievous conduct, to theft, and to dirty and slovenly habits, he was noisy and destructive, his perception and memory were impaired, his conversation and conduct irrational.<sup>1</sup> When admitted here, three months after the onset of insanity, he was in much the same condition, except that he was more apathetic and generally refused to answer any question. There was a doubtful cicatricial trace of chancre, the cervical and inguinal glands were amygdaloid, and there was a mucous patch on the buccal membrane. The hard palate was high and narrow; the face flushed very readily; the patient was somewhat emaciated by his attack, and the viscera were healthy. Under mercurials, with cod liver oil and mineral acids, together with frequent warm baths and cold douche to the head, he improved both mentally and physically for a month, but then suffered from a relapse in which his demeanour was most grotesque, and his attention often seemed to be absorbed by imaginary objects, and, as the result of his hallucinations, he often appeared to be on the brink of some impulsive outburst. After twelve days treatment by warm baths, cold to head, aperients, chloral hydrate and potassic bromide, he was very decidedly better, and now complained for the first time of having had severe cranial pains during the relapse. He had also again grown much thinner. Iodide of potassium was now given with the oil and mercurial. After this he improved steadily; all traces of syphilis disappeared, and he was finally discharged recovered about five months after his admission, having gained about 40 lbs. in weight whilst under treatment here. After recovery he remembered nothing of the epileptic fit, and denied ever having had epilepsy, but attributed the attack of insanity to a bout of drinking in which he indulged for a week.

In this case there were the previous attack of insanity; a defective cranial conformation; the possible action of the two causes—syphilis and a drinking bout;—an attack of epilepsy in one who had never, as far as known, suffered from epilepsy before; and then mania. The mania followed the epilepsy closely; the epilepsy was extremely early for syphilitic epilepsy, and this seems, therefore to be a case in which the mania intercurrent in early secondary syphilis was not dependent upon syphilis. Nor is this view confuted by the recovery from the mental and the syphilitic symptoms under the treatment adopted, which was only in part specific. The primary lesion appears to have been of the typical Hunterian kind.

CASE 2.—A cavalryman, aged 26, was admitted for his first attack of insanity of three months' duration. It had developed

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<sup>1</sup> Before admission he had been treated at first with mercury and then with the iodide.

suddenly with active maniacal symptoms ; these remitted, but exacerbations of great excitement occurred from time to time, and he became possessed by extraordinary delusions,—some, of corporeal injury, and others of personal rank. This state continued when he was admitted. There was the cicatricial trace of a chancre, which had been contracted eighteen months previously, and had been followed by hard non-suppurating swellings in the groin ; on the arm were psoriasis patches, and he complained of pain across the temples and the chest. The palatine arch and the teeth were deformed, the face was flushed, and the second cardiac sound was somewhat accentuated. He was ordered Hydrarg. cum Cretâ in medium doses and to take a night draught of Chloral Hydrate. He improved very quickly ; at the end of twenty-six days the chloral was omitted, the mercurial being continued, but five days afterwards he became worse, though he did not relapse entirely into the former condition. Chloral was resumed and was taken for five weeks, at the end of which time he could sleep without it and was greatly improved. From this time he continued to gradually regain mental sanity and was finally discharged recovered, having been subjected to a mild mercurial course of four months duration.

In this case the mania was intercurrent in later secondary syphilis, and there was a moral cause which might have been adequate to produce mental disorder in one predisposed to it, for he had been reduced from the position of sergeant, having been thought too young for the duties. Any family predisposition, however, was flatly denied by the friends. The general conclusions appended to Case 1 apply here also.

CASE 3.—An infantryman, aged 26, when admitted was the subject of insanity of ten months duration. It had developed at first insidiously, but soon assumed the form of active maniacal excitement and turbulence. Later on, and when admitted, he was moody, irritable, and suspicious. There were some spots of a disappearing scaly rash. Twelve months before he had had chancre followed by a cutaneous rash, by sores on the tongue, and by much headache. Mercury was prescribed when he was admitted, but at times he refused to take it, and there was no special change in his mental condition until ten months afterwards, when a decided maniacal relapse took place, in which he was restless, wild, and gesticulating violently, grimacing, and often contorting his features into a fiend-like expression, and kept reiterating his incoherent utterances in a remarkably changed tone of voice, and refused food. Varied treatment was employed, but several weeks elapsed before he returned to the condition which existed previous to the access of these symptoms, and the return was interrupted by a second exacerbation. Latterly, he has again

had specific treatment, as he complained from time to time of headache, muscular pains, and tenderness of tibiae. He has improved considerably and there is now some hope of sufficient improvement to warrant his discharge eventually.

The relapses which occurred after specific treatment of the syphilis present are an indication that in this, as in the other cases, it is very doubtful whether the syphilis can be accepted as the cause of the maniacal outbreak, although the history of the onset is just such a history as those rely upon who think that they frequently find a *causal* relationship between the secondary syphilis and the acute insanity of their patients.

The other cases had somewhat similar histories and need not be given in detail.

TABLE.

No.	Age on Admission.	Duration of Insanity on Admission, and Mental Symptoms before and at Admission.	History and Symptoms of Syphilis, and the relation in time to Mental Disorder.	Complications and Remarks.	The Associated Phases of the Two Diseases.	Termination of Case.
1	36	Six months' duration; second attack; first attack was ten years previously. In second attack, had at first mania, alternating with melancholia. Later, the mental state varied, and at different times he was morose, sullen, depressed, or restless and excited. Refused food, was extremely emaciated and feeble.	(1) Syphilitic cachexy, scars of serpiginous ulceration over shins. (2) Node on right parietal bone, severe cranial cranial and tibial pains, irregularity of tibiae.	Incipient phthisis pulmonalis.	Insanity, supervening on tertiary syphilis.	Discharged. Recovered.
2	26	Three months' duration; first attack. Developed suddenly as acute mania, subsequently became chronic mania, with active excitement and extraordinary delusions.	History of chancre eighteen months before insanity, followed by hard, indolent non-suppurating swelling of inguinal glands, and by cutaneous rashes.	Congenital pre-disposition? Defective cranial development.	Mania, intercurrent in secondary syphilis.	Discharged. Recovered.
3	22	Three months' duration; second attack. Epileptic attack five months after primary syphilis, followed by acute maniacal and suicidal symptoms, subsequently maniacal, improved, relapsed, recovered.	History of syphilis, primary, and of its treatment during fifty-six days. Amygdaloid cervical and inguinal glands, faint scar on penis, mucous patch in month; afterwards nocturnal headache, insomnia, &c.	Hereditary pre-disposition? Complicated by effects of intemperance.	Mania, intercurrent in secondary syphilis.	Discharged. Recovered.
4	22	Ten months' duration; first attack; developed insidiously. (1) Active maniacal excitement and violence. (2) Chronic mania and mental impairment, irritability and peculiar demeanour. (3) Recurrent maniacal symptoms with fantastic actions. (4) Convalescence.	History of primary and of secondary syphilis; the primary infection had preceded the onset of insanity by two months. Headaches, syphilitic patches on the skin. Later, occasional headache, muscular pains, tibial periostitis.	Relapses.	Mania, intercurrent in secondary syphilis.	Improved.



No.	Age on Admission	Duration of Insanity on Admission, and Mental Symptoms before and at Admission.	History and Symptoms of Syphilis, and the relation in time to Mental Disorder.	Complications and Remarks.	The Associated Phases of the Two Diseases.	Termination of Case.
5	27	Eight months' duration; first attack. (1) Depression and delusions on religious subjects. (2) Excitement, emotional distress, delusions and hallucinations on religious and other subjects. (3) Delusions as to his body, recovery of emotional control.	History of frequent venereal infection, of syphilitic rashes three or four years before, and of occasional headache since. On admission, a few coppery squamous patches, and traces of former rash (cicatricial). Inguinal glands somewhat amygdaloid, dull sallow pallor.	...	Syphilis preceding insanity.	Improved.
6	31	Nine months' duration; first attack. (1) Delirium tremens, followed by morose mania and suicidal tendencies. (2) Absurd and extravagant delusions, suspicious, insolent. (3) Quiet, with absurd delusions, but occasionally impulsively violent if annoyed by others.	History of frequent venereal infection. On and after admission, syphilitic signs from time to time; as superficial ulcerations of mucous membrane of mouth, cheeks, tongue, and fauces; condylomata of anus and scrotum; adenitis, and at first amygdaloid glands. Had had condylomata previous to admission.	Complicated by recent soft venereal sores.	Insanity, with late secondary syphilis.	Improved.
7	30	Eighteen months' duration; first attack. (1) Mental depression. (2) Excitement, delusions. (3) Irrational, extraordinary delusions, incoherence, emotional disorder and hallucinations of sight.	History of frequent venereal sores; and of syphilis. Cicatrices in groin and on penis; scaly cupreous patches on trunk and limbs. Early amygdaloid glands.	Hereditary predisposition?	Insanity, with late secondary period, and "period of latency" of syphilis.	Still under care.
8	21	"Of some months' duration" when admitted nine years ago. (1) Of insidious origin, and preceded by mental weakness. (2) Religious and hypochondriacal depression; hallucinations of hearing and of touch; extraordinary delusions. (3) Secondary dementia.	History imperfect. On admission, suffered much from nocturnal headache and insomnia. Tibiae irregularly nodose; cicatrices from former ulcerations on legs. Latterly, dull sallow pallor, and tertiary ulcerations of the skin.	...	...	Ditto.

9 46	Four months' duration; first attack. (1) Suicidal attempt under mental depression, and the delusion that he was robbed. (2) Ordinary melancholia of chronic form.	History from patient of hard sore about fourteen years previously, of which a cicatricial trace remained, followed by muscular pains, and iritis, of which traces remained also. After admission, nocturnal cranial pains, with tenderness. Insomnia, vertebral tenderness. Later, nocturnal pains of cranium and of limbs; insomnia, &c.	Cardiac hypertrophy; phos- phaturia; af- fects of long tropical ser- vice.	Melancholia, inter- current in tertiary syphilis.	Ditto.
10 30	Twelve months' duration of marked in- sanity, but always, or for a long time "peculiar." First attack, insidious. (1) Suicidal attempt; melancholia; hallucinations of sight and hearing; delusions of designs on his life, and tor- ture by mysterious agencies. (2) Later, "clocks tick in his head," and old com- rades follow him as "spirits." (3) Ob- stinate, mischief-making; no depression; persistent hallucinations, &c.	History of syphilis.—History from patient of venereal sores on several occasions; once, two years ago, followed by sore throat; and of severe cranial pains during the year preceding his admission. Cranial pains after admission, cured by specific treatment.	Incipient phthisis pul- monalis, arrested by treatment. Hepatic en- largement. "Predisposi- tion." (?)	Melancholia, inter- current in sec- ondary syphilis.	Ditto.
11 37	Marked insanity for five months, but really of long duration and insidious develop- ment. (1) despondency, suicidal ten- dencies; hallucinations of sight and hearing; delusions of persecution, and of conspiracy against him. Impairment of perception and judgment. (2) Im- proved; then (3) strong delusions and ex- citement about the hostility of those about him, with severe cranial, &c., pains.	History of syphilis, of periosteal nodes on both tibiae, sternum, and right parietal bone, of venereal rash, of which some cicatrices remained, and of epileptiform seizures. The chancre and secondaries occurred about four years before. On admission, cranial periostitis, severe cranial and tibial pain, especially at night; insomnia, remains of nodes of tibiae. Improved for a time, then at- tacks in which his delusions and hallu- cinations became vivid, with intense frontal and vertical pain, and tender- ness; dissipated by large doses of K. I. Latterly, coppery scaly patches on calf of right leg, &c.	Climatic influ- ences. Hepa- titis.	Insanity (early melan- cholia), intercur- rent in late sec- ondary, and early tertiary, syphilis.	Ditto.



16 36	him at night; at times refused food. (3) Morose, fearful, suspicious; absorbed by his fancies, usually reticent; incoherence, general mental impairment. Three months' duration; first attack. Is said to have suffered from the effects of tropical climate and intemperance, from an attack in which he lost control of the sphincters; and from meningitis (?). Later on, dementia, inco-ordination of movements, physical signs of general paresis.	History of sores contracted frequently, of ulcerating late syphilitic rash, of tibial periosteal nodes. On admission here, a faint scar of chancre, cicatrices left by the rash, and irregularity and tenderness of the tibiae.	Incipient phthisis pulmonalis. Enlarged spleen. Physical signs of G. P.	Syphilis, with signs of general paresis.	Ditto.
17 35	Four months' duration; first attack; eccentric for a long time previously. Had an attack of epilepsy eight years before. at first, exalted delusions; incoherence; filthy and destructive propensities. Later on, <i>bien être</i> ; extravagant delusions; at times depressed; morose. The course varied by epileptiform attacks. Latterly, demented; speechless, feeble.	History of primary and secondary syphilis; periosteal nodes, &c. Nodes after admission, leaving behind them thickening and irregularity of the periosteum of both tibiae.	Intemperance.	Syphilis and general paresis.	Ditto.
18 27	Maniacal outbreak undergoing remissions and exacerbations. (2) Incipient general paresis. (3) Hypochondria paralytica. (4) Temporary improvement. Two and a half years' duration; first attack. (1) Delusions of mysterious adverse electrical influences exerted upon him. Hallucinations of sight and hearing, morose; occasionally excited, but only in the earlier periods.	Duaky syphilide on admission. Other cutaneous patches later; large and hard inguinal glands; once slight right facial paralysis (Traces of chancreoids and bubo). History of old primary syphilis, followed by rashes. After admission, some scaly patches (cutaneous), episternal tenderness. Later, localised nocturnal cranial pains, subacute arthritis; (slight trace of chancre).	Hereditary predisposition. Unilateral sweating. Phthisis. Pneumonic phthisis.	Secondary syphilis in a subject of general paresis.	Discharged.
19 27				Insanity, with early tertiary syphilis.	Died of phthisis.

No.	Age on Admission.	Duration of Insanity on Admission, and Mental Symptoms before and at Admission.	History and Symptoms of Syphilis, and the relation in time to Mental Disorder.	Complications and Remarks.	The Associated Phases of the Two Diseases.	Termination of Case.
20	31	Of thirteen months' duration, when admitted in 1869. During the earlier periods there were maniacal symptoms with exalted delusions. Subsequently, he was incoherent and excitable. Finally, he never uttered a word for two years, under delusions about the effects of his speech.	Remains of extensive syphilitic disease of liver and spleen. Liver fatty and waxy, 80½ ozs.	Cirrhosis; caseation of lung. Intestinal ulceration. Enlarged intercostal glands. Pyloric thickening. Granular kidneys.	Chronic insanity, co-existing with syphilitic sequelæ.	Died.
21	26	Eight months' duration; first attack; insidious in origin, preceded by frequent syphilitic symptoms, to which it was attributed. (1) Irrational, muttering, hoarding rubbish, obstinate, morose, of wandering and solitary habits, easily roused to great excitement and violence. Subsequently, chronic mania, but good tempered, volatile, incoherent.	History of syphilis. After admission, pupular rash, ulceration over right parietal bone. Nodes on tibia and ulna, the latter followed by caries of bone and by ulceration, finally leaving depressed cicatrices. Same over lower maxilla. Thickening of tibia. At post-mortem, besides traces of former osteitis and peritonitis, were gummata of liver, spleen, and kidney (some doubtful lung and lymphatic lesions, besides the phthisical).	Phthisis pulmonalis.	Insanity, with syphilis.	Died of phthisis.
22	35	Sixteen months' duration; first attack. Died six and a half years after admission. At first, delusions of plots against him, hallucinations of sight and hearing, and supposed suicidal tendencies. Later, very restless; chronic mania, strange delusions and hallucinations of sight.	Caries of various bones and cicatrices of former rashes. Post-mortem indications of past gummatous disease of spleen. Lardaceous spleen and kidneys; fatty and nutmeggy liver; calvarium dense.	Cirrhosis and caseation of lung; and spinal caries, destruction of cord, &c.	Chronic insanity (eight years), with traces of past syphilis.	Died.

23 32	More than eighteen months' duration; first attack. Suicidal attempt, eccentric, gloomy, solitary, apprehensive; a variety of delusions; hallucinations of hearing. Later, quiet, delusions, &c., persistent.	History of syphilis many years before; cicatrices of bubo and of rashes; sarcocele. Calvarium dense; remains of gummata of spleen, and of former adhesive perisplenitis; also of former adhesive perihepatitis. Atheroma of coronary arteries and aorta.	Intemperance. Tropical climate (effects of).	Chronic insanity (two years), with traces of past syphilis.	Died of acute disease.
24 38	Seven months' duration; first attack. (1) Delusions, excitement, incoherence; delusions extravagant, excitement occasionally vehement. (2) Absurd delusions; morose; at times threatening; general paresis.	History of syphilis whilst in the service. Coppery patches on back; subsequently ulcer on leg of tertiary type; cicatrices on body, legs, and in groin. Certain of the changes found post-mortem in dura mater and calvarium, doubtfully attributable to syphilis.	...	Tertiary syphilis. General paresis.	Died.
25 36	Six months' duration; first attack. (1) Delusions, excitement, noisy, restless, disposition to violence. (3) Absurd and extravagant delusions, mental feebleness. (4) Dementia.	Syphilitic ulcers on legs; keloid on body; rapid effect of iodide of potassium upon ulceration.	Suppurative nephritis.	Tertiary syphilis. General paresis.	Ditto.
26 33	Five months' duration of marked features, but of insidious origin. (1) Fatuity, general paresis, delusions of annoyance and impending danger. Later, depressed, lachrymose, fatuous.	History of syphilis, date not ascertained; sequelae of double iritis. Early neoplastic growth in spinal meninges, and very slightly in cerebral pia-mater; (faint of cicatrix bubo and of primary sore).	Hereditary pre-disposition to insanity.	Syphilis. General paresis.	Ditto.

## Chronicle of Medical Science.

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### REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By ROBERT HUNTER SEMPLE, M.D.,

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*On the Serum of Blood as a Therapeutic Agent.* By FRANCIS VACHER, Medical Officer of Health, Birkenhead. 1876.—Mr. Vacher's attention appears to have been directed to the therapeutical use of the serum of blood by the beneficial effects said to be produced in France and America by the use of blood itself in the treatment of some diseases. It seems, in fact, that in Paris patients resort to the slaughter-house every morning to drink the still fuming blood of the oxen slaughtered for the table. Mr. Vacher observes that for various reasons this practice would be impossible in England, and he suggests that the serum of the blood possesses most of the nutritive and curative properties of the blood itself and therefore might be used as a therapeutic agent with advantage. From his position as Medical Officer of Health it came to his knowledge that the serum was held in high repute by the journeymen butchers employed in the slaughter-houses, being considered as a tonic in scrofula and almost a specific in intestinal worms. Mr. Vacher was able to obtain supplies of the serum from time to time from the slaughter-houses under his supervision, and he has supplied some bottles of it to his professional friends who have employed it with advantage, chiefly as a vermifuge and as a substitute for cod-liver oil. As an anthelmintic he is informed that the serum should be administered in the morning fasting, the dose being from one to two table-spoonfuls for a child, and a large wine-glassful for an adult. As a food-medicine, one ounce of the serum should be taken two or three times a day, the best time for taking it being probably about an hour before meals. Mr. Vacher gives a caution against the danger of using the remedy when it is not perfectly sound; but, as fortunately the commencement of the decomposition is at once manifest by the smell, it is not likely that such a mistake would occur.

*On the Therapeutical Uses of the Bromide of Camphor.* By Dr. L. PATHAULT, of Paris.—Bromide of camphor was discovered and described in 1862 by Swartz, of Ghent, and M. Deneffe, of the same

town, first introduced it into practice as a sedative to the nervous system, and it has since been employed with success in France and England and in America. As to its physiological characters, it appears, from the experiments which have been made, to have the power of reducing the number of cardiac pulsations, as well as the respirations, and of lowering the temperature; it also possesses hypnotic properties. As a therapeutical agent, the bromide of camphor has been employed with considerable success by Deneffe, Lawson, Hammond, Charcot, Bourneville, Desnos, Potain, Siredey, Tommasi, Lorain, Vulpian, Raymond, and others. The patients who derived the greatest benefit from its use were those who suffered from the diseases generally classed as nervous (*les névroses*), including very different affections, as dyspnœa, heart-affections, nervous disturbances of the genito-urinary organs, &c., and the beneficial results corresponded with the physiological properties of the drug. Dr. Pathault states that fresh researches are necessary to determine its therapeutic value with precision. In the pamphlet in which he treats of the bromide and its different properties he records a series of observations on its therapeutical applications. The first in the series of affections in which it has been given is delirium tremens, the dose being about seven grains and upwards frequently repeated; and in succession its effects are recorded in insomnia, in which it has been partially beneficial; in chorea, in which it was more successful; in hysteria, in which the results were encouraging; in epilepsy, in which the fits were reduced in number and vertigo was relieved; in dyspnœa, in which it was partially successful; in neuralgia, in which its effects were doubtful; and in a few other maladies.

*On the Absorption of Iodine by the Cutaneous Surface in Children.* By Dr. JULES SIMON, and M. PAUL REGNARD, of Paris.—Some little girls affected with ringworm of the scalp were treated in a very simple manner by swabbing the whole of the head with a mixture composed of equal parts of iodine and glycerine, and the cases were improving, when, in about a month, one of the children was taken ill with symptoms of iodine poisoning. The urine, being examined, was found to contain iodine, and a similar examination being made in the case of the other children, the results were the same. All the children therefore had absorbed iodine, but only the first mentioned had presented any general symptoms of iodism. In half the cases, too, the urine contained albumen. Experiments were now made in a methodical manner, and the results are given in the paper now under notice. Care was, of course, taken to employ appropriate tests for the iodine, and care was also taken to eliminate albumen as far as possible from the food consumed. The general results were that in fourteen children treated by the external use of iodine, all presented this substance in their urine, and the quantity found varied according as the applications to the scalp were diminished or suspended. When the surface covered with the application did not exceed the diameter of a five franc piece, the iodine entered but little or not at all into the urine, and albumen was never



found. The absorption by the skin therefore requires, in order to be appreciated in a short time by the examination of the urine, rather large surfaces of application; but it is suggested that smaller quantities might perhaps be detected in the saliva than they would be in the urine. The conclusions drawn are that iodine applied to the skin passes readily into the blood in children, these being the subjects on which the experiments were made; the absorption in one of the cases was twice followed by symptoms of iodism; and in half the cases there was albuminuria, sometimes of a marked character.—*L'Union Médicale*, July 6th, 1876.

*On the Employment of the Nitrite of Amyl in Epilepsy.* By Dr. BOURNEVILLE, of Paris.—At the conclusion of an article on the use of the nitrite of amyl, Dr. Bourneville observes that this substance not only produces important modifications in the circulation, but also probably in the composition of the blood. The lowering of the temperature is produced in the lower animals as well as in man by the nitrite. The end of the epileptic attack is announced, in cases where the nitrite has been inhaled, by movements of deglutition and by nausea and vomiting, and it is stated that an attack cannot be considered as having really passed away unless efforts at vomiting or vomiting itself have been caused by the inhalation. Generally speaking, when the inhalation has been properly performed, the patients, when once restored, did not suffer from a return of their attacks on the same day. Some of the observations showed that tolerance was easily established, so that the dose required to be increased at each inhalation. Dr. Bourneville has no doubt that the nitrite produces a well marked effect in attacks of epilepsy, but he is not sure that it exercises any influence on the progress of the convulsive symptoms, that being a question for the solution of which new facts are required. One of the cases described, however, remained eight weeks without any attacks, and another presented no fresh attacks for four months after the inhalations had been prescribed.—*Gazette Médicale de Paris*, Aug. 12th, 1876.

*On the Properties of the Xanthium Spinosum and its Employment in Rabies.*—In the *Journal de Thérapeutique*, the Editor (Professor Gubler) inserts a letter received from Dr. Grzymala, of Podolia, pointing out the therapeutical properties of a hitherto neglected plant, the *Xanthium Spinosum*, which the writer describes as a cure for rabies. Dr. Grzymala states that he has employed the xanthium for a number of years with the greatest success, and indeed he has never observed a single case where it has failed, although he has employed it at least a hundred times both for the lower animals and for human beings who had been bitten by mad dogs and wolves. In the country where he lives, rabies is very frequent, and for more than twenty years since he has been in practice there, he has seen, on the average, ten cases a year. The physiological effects of the plant appear to resemble those of the jaborandi, being sudorific, sialogogue, and slightly diuretic; but the action is less marked than that of jaborandi and the symptoms do not all appear at the same time. Some patients perspire, others are salivated, and there

are some who pass more than the natural quantity of urine. The temperature is slightly raised, and the circulation is generally accelerated a little under the influence of the plant. Some complain of headache, others of nausea, and some even vomit the first dose. The dose for an adult is 60 centigrammes ( $\frac{1}{10}$ ths of about 15 grains) of the dried powder of the leaves of the xanthium, repeated three times a day and continued for three weeks. About twelve years ago one of Dr. Grzymala's dogs, being seized with rabies, bit a cow, a pig, a dog, a cat, and a tame crane. The cow, the pig, and the dog were subjected to the treatment for three weeks, and they all escaped the disease, while the crane and the cat both died of hydrophobia. During the Crimean war, a family, composed of twelve persons, had been bitten by a mad wolf, and six of these being admitted into the hospital under Dr. Grzymala's care were all cured, while the six others, treated in other ways, all died hydrophobic in from twelve to sixty days. The writer adduces many other cases where the drug was administered with apparently equal success, and he goes so far as to state that since he has ascertained the properties of the xanthium, he never cauterises the bitten part and has no longer any fear of the consequences.

*Some Critical Remarks on the Employment of Iron in the Treatment of Chlorosis.* By Dr. DUJARDIN-BEAUMETZ.—Dr. Dujardin-Beaumetz entertains some doubts as to the utility of ferruginous preparations in the treatment of chlorosis, and he gives reasons for his scepticism. Before admitting, he says, that in the work of organic reconstruction iron is superior to other medicines, we ought to see the quantity of iron which disappears from the economy as a consequence of anæmia. Take for example a young girl of the weight of 60 kilogrammes (a kilogramme is rather more than 2 lbs.). According to the researches of Boussingault the proportion of iron would be, in relation to the weight of the whole body, represented as '00011, which would give, in the case of the girl, 5'454 of iron. But the iron is distributed in various parts, and the blood contains only '5068 of iron in 1000 parts so that the quantity of iron contained in the girl's blood would be about 2 to 2½ grammes (a gramme is about 15 grains.) But this quantity does not belong exclusively to the globules, a certain part being distributed to the albumen and the fibrine, and thus the quantity reserved for the globules is diminished. These last alone undergo a more or less marked diminution in chlorosis, but the deficiency never reaches more than from a quarter to a third of the total amount of the globules, so that in admitting the numbers in the given case it is found that the diminution of iron in this disease is very small and is represented by figures varying between 10 and 50 centigrammes at the utmost. This small loss of iron is restored every day by the food. The arguments thus adduced shew that the ferruginous preparations may act in chlorosis, not by replacing the iron which has disappeared, but in stimulating the digestive functions and promoting nutrition and assimilation. Dr. Dujardin-Beaumetz does not deny the beneficial effects of iron in chlorosis, but he thinks that the results have

been much exaggerated, and that, in a great number of cases, hygienic measures have been superior to the iron treatment. He adduces his own experience in proof of this view, and he states that in the case of several young women in a school which he attended, and who were suffering from chloro-anæmia, the symptoms were not at all relieved or improved by iron, but were cured by the introduction of hydrotherapeia and gymnastics in the institution where the patients were being educated.—*Bulletin Général de Therapeutique*, May 15th, 1876.

*On the Therapeutical Properties of the Myrtle.* By Dr. DELIOUX DE SAVIGNAC.—The chief chemical constituents of the common myrtle are an essential oil and tannic acid. The latter exists in such quantity that the myrtle is used in some countries for tanning, for dyeing black, and for making ink. Essence of myrtle is obtained chiefly by the distillation of the leaves, and it possesses the characteristic smell of the plant. There exists, besides, in the myrtle berries a fatty oil which was used by the ancients for numerous medical purposes, as is stated by Pliny. Dr. de Savignac considers that for medical use the leaves and the berries ought to be principally employed, as they are more active than the flowers, containing, in fact, a larger proportion of essential oil besides tannic acid. He thinks that, over and above these two constituents, the myrtle contains a bitter principle and one or more resins. He administers the plant in infusion of the leaves and the berries, powder of the leaves, and tincture. Dr. de Savignac first employed the preparations of the myrtle in leucorrhœa, and vaginal injections of this kind have been prescribed by him with great success. Being of the nature of aromatic astringents, these agents have been also found useful in prolapsus of the womb and of the rectum, and also in strengthening the genital parts after laborious confinements. He does not regard the myrtle as a panacea in leucorrhœa, but he thinks it superior to many other agents in the treatment of this complaint. He has employed these preparations also for recent wounds, which, when covered with myrtle powder, dry and cicatrise with great rapidity; and in suppurating sores the infusion of the berries or leaves diminishes the suppuration, destroys fœtor, and promotes granulation. The myrtle has also been employed with good effect as a tea in bronchial catarrh attended with obstinate cough. Dr. de Savignac concludes his paper by recommending a trial of the preparations of the myrtle, which, he thinks, possess real value among the bitters and aromatic stimulants and the astringent tonics.—*Ibid.*, Feb. 29th and March 15th, 1876.

*On the Treatment of Epilepsy by Sodid Bromide (Bromide of Sodium).* By Dr. W. A. HOLLIS, Assistant-Physician to the Sussex County Hospital.—Dr. Hollis has, in a former year, recommended the use of bromide of sodium in the treatment of nervous diseases, and he has had several opportunities lately of testing the efficacy of this salt in epilepsy, and he now gives the result of his experience. The number of his cases is eleven, and the ground of their selection has been that the epileptic attacks previously to treatment were at

very short intervals, and any amelioration in their number or character was easily observed. As regards the females, Dr. Hollis has carefully excluded pure hysteria from the category, and mostly relied upon the fact that during the paroxysms the patients bit their tongues or otherwise did themselves some severe bodily injury in addition to manifesting the ordinary phenomena of the attack. In order to test the efficacy of the treatment, Dr. Hollis sometimes omitted the ordinary bromide mixture for a week or more during the management of the case, and then in most instances he found an increased frequency in the number of the fits. It is not very clear, from Dr. Hollis's cases, whether he has found the sodic bromide superior in its efficacy to the potassic, but in one case he states that the use of the former was attended with beneficial results while the latter had been comparatively useless. Dr. Hollis observes that the bromide appears to exert a greater influence on the so-called "convulsions" of children than it does on the more confirmed epilepsy of adult age. Dr. Hollis's plan of treatment appears to be to give the salt in large doses (as, for instance, twenty, thirty, forty grains) at frequent intervals. The analysis of the eleven cases shows favorable results, the fits diminishing remarkably in number after the treatment.—*British Medical Journal*, July 1st, 1876.

*On the Hydrobromate of Cicutine (Conia) and its Therapeutic Uses.* By M. MOURRUT, Pharmacien, of Paris.—All chemists have agreed in regarding the salts of cicutine as unstable or difficult to crystallise, or so hygroscopic as to forbid their employment. M. Mourrut, however, has succeeded in obtaining crystals of the hydrobromate, made by combining directly hydrobromic acid with the alkaloid. This salt crystallises in colourless prismatic needles, very soluble in water and alcohol, and less so in ether and chloroform; the crystals are not deliquescent and are inodorous, and have but little taste. They contain about a third of their weight of bromine. M. Mourrut made several experiments on the lower animals, and found that the effects were to produce paralysis and sleep. During the greatest depression, however, the respirations and the beats of the heart were distinctly perceived, and there was no serious impairment of the sensation, for the animal always felt when it was pricked. The observations made on patients were communicated to M. Mourrut by Drs. Saison, Landur, and Regnault. The first of these physicians gave a child three years old five milligrammes ( $\frac{1}{100}$ th of about fifteen grains) of the hydrobromate every hour, and although it was suffering from whooping-cough it slept all night without coughing. Dr. Landur gave the salt in whooping-cough, asthma, the cough of phthisis, and the pains of dentition in children, and the results were satisfactory. He gave it in doses of two milligrammes for children of one year old, and one centigramme for adults. Dr. Regnault employed it in hypodermic injections, in the case of a man thirty-five years old, affected with sciatica, and after the second injection the pain disappeared. He also injected three milligrammes of the salt in a phthisical patient, aged twenty-three, suffering from intercostal neuralgia, and the pain was relieved. M. Mourrut concludes

his communication by observing that different specimens of cicutine appear to produce varying effects, some being much more poisonous than others, but the salt which he has prepared produces circuitism and its action is always the same.—*Bulletin Général de Thérapeutique*, May 30th, 1876.

*On the Physiological and Therapeutical Properties of the Salts of Cicutine (Conia), and particularly of the Hydrobromate.* By Dr. DUJARDIN-BEAUMETZ, of Paris.—In this paper Dr. Dujardin-Beaumetz, attributing to M. Mourrut the discovery of a stable salt (the hydrobromate) of cicutine, and reproducing that writer's description of its characters, enters into greater details as to its physiological, chemical, and therapeutic relations, as well as those of cicutine (conia) and of hemlock in general. Cicutine and its salts appear to produce on the nervous system phenomena identical with those of curare, but according to the researches of some modern physiologists the pneumogastric nerve is affected by the former while it is unaffected by the latter. In reference to the action of the hydrobromate of cicutine on man, Dr. Dujardin-Beaumetz quotes Dr. Saison as giving the results of some interesting observations made on himself, by which it appears that after taking fifteen centigrammes ( $\frac{1}{10}$ ths of about fifteen grains) he fell asleep in an hour, and did not wake during the night and did not cough, although he had slight bronchitis at the time. The next morning at seven o'clock he took another dose of the same amount, and at nine the effects began by a tendency to vertigo, some little reeling, or at least uncertainty of gait, together with some disturbance of the intellectual faculties, and although there was no real sleep there was a kind of tranquil intoxication. There was no alteration in the secretions, either renal or salivary, no dryness of the throat, and no disturbance of the general sensibility or power of movement. At three o'clock, after taking some coffee, the symptoms gradually disappeared.

Dr. Dujardin-Beaumetz concludes by offering some observations on the principal therapeutical applications of hemlock and of cicutine and its salts. Rejecting many of the supposed properties of these bodies, the author thinks that they may be employed in convulsive affections, and particularly for reflex symptoms connected with the pneumogastric nerve. Thus he thinks they may be usefully administered in convulsive cough, asthma, whooping-cough, in certain forms of hiccough, in dysphagia, vomiting, &c. In asthma he thinks hemlock is especially beneficial, and he would also recommend it in the convulsions of tetanus, the convulsions of infancy, in chorea, and in neuralgia. Of all the methods of employing the salts of cicutine, and in particular the hydrobromate he thinks the hypodermic method the best. Hemlock, in fact, appears to act very differently according as it is administered by the stomach or is introduced beneath the skin, and the intestinal juices seem to destroy, in a certain degree, the physiological and toxic properties of the drug. Hence a very small quantity used hypodermically will produce a more remarkable effect than a considerable dose taken by the mouth.—*Ibid.*, July 15th, 1876.

*Some Further Remarks on the Modus Operandi and the Therapeutical Value of Jaborandi.* By Dr. SIGMUND PURJESZ, Jun., Assistant Clinical Professor in Professor Wagner's Hospital Wards in Budapest.—Dr. Purjesz has previously published some observations on the action of Jaborandi, founded on the history of a case of chronic parenchymatous nephritis treated with that drug. The results of the case led him to draw several conclusions as to the effects and therapeutical value of jaborandi, and on the whole he convinced himself that it was not a suitable remedy for the disease just mentioned. He now details the history of four more patients treated in Professor Wagner's wards with jaborandi, two of them suffering from contracted kidney, one from insufficiency of the mitral and relative insufficiency of the tricuspid valve with marked ascites, and one from psoriasis. In all the cases a careful record was kept of the weight of the body, the temperature, the rate of the pulse, and the condition of the urine, as well as all other particulars, including the sweating and the discharge of saliva. The results of the action of jaborandi were not very striking, for in the cases of dropsy from kidney disease the fluid disappeared without any therapeutical measures, or the treatment was commenced while it was disappearing. In answer to the question whether jaborandi exercises a beneficial operation on the dropsy accompanying heart disease, Dr. Purjesz gives a decidedly negative reply. In skin diseases jaborandi seems to cause no change. From the result of all the observations made by him, both now and formerly, Dr. Purjesz considers jaborandi as an unsuitable drug in the treatment both of heart and kidney diseases attended with dropsy, and he thinks that the properties of the plant can only be duly appreciated when successful efforts have been made to procure and to isolate its active principles, and to avoid altogether the other constituents which may have an injurious operation. — *Deutsches Archiv für Klinische Medicin*, May 26, 1876.

*On the Successful Treatment of a case of Meningitis by Iodide of Potassium.* By Dr. BONAMY, of Nantes.—A child, aged 7½ years, previously in good health, was suddenly seized, in December, 1869, with violent pains in the head, vomiting, and delirium. There was unconsciousness and great acceleration of the pulse. Some leeches were applied behind the ears. In twenty-four hours coma supervened and lasted more than a month, after which time Dr. Bonamy was called to see the child in consultation. Calomel had been given in purgative doses and blisters had been applied to the extremities. On the 16th January, 1870, the coma was so profound that the patient could not be roused; there was divergent strabismus with dilatation of the pupils, which did not contract by a bright light; grinding of the teeth, tetanic stiffness of the muscles of the neck and of the posterior part of the trunk; respiration difficult and almost stertorous. The pulse was very weak, slow, and unequal; obstinate constipation: great emaciation. Dr. Bonamy prescribed calomel in divided doses, 1 centigramme ( $\frac{1}{100}$  of about 15 grains)

every hour; mercurial frictions on the shaven scalp; and quinine wine and broth to be administered as food.

During the month of February the patient remained in the same state, and the treatment was pretty much the same, except that small doses of *nux vomica* were tried but abandoned, owing to their unfavorable effects.

On the 5th of March there was no marked change, the coma being the same, with strabismus and dilatation of the pupils, and weak, slow, and unequal pulse. The iodide of potassium was now prescribed in doses amounting to 50 centigrammes (about  $7\frac{1}{2}$  grains) every day.

On the 8th of April, namely, rather more than a month after this treatment had been adopted, Dr. Bonamy was astonished at the improvement in the patient, whose countenance now began to show intelligence, and who was able to answer questions in a rational manner; and other symptoms indicated amendment, though there still remained incontinence of the urine and the stools. The coma had lasted more than two months, and the iodide of potassium had been regularly taken ever since the 5th of March. This salt was ordered to be continued, and food to be given, for which the patient began to feel some appetite.

During the month of May the improvement continued, and the child was able to sit in an easy chair, and also to use her limbs, and to control her evacuations. Some months afterwards she was able to walk with crutches, but eventually she walked without them.

Four years afterwards Dr. Bonamy saw the patient at his own house. She was then  $11\frac{1}{2}$  years old; her face had a bright and animated appearance, and her intelligence was that of a child of her age. The only symptom remaining was a little weakness of the left lower extremity. Dr. Bonamy remarks that the symptoms at the outset, namely, violent headache, vomiting, extreme acceleration of the pulse, delirium, and constipation, clearly indicated cerebral meningitis, and the diagnosis was subsequently confirmed by the persistence of the delirium, the irregularity and slowness of the pulse, the convulsions, the strabismus with permanent dilatation of the pupils, and coma. Dr. Bonamy regards the case as one of simple as contradistinguished from tubercular meningitis, and he draws attention to the good effects apparently produced by the long-continued use of the iodide of potassium, the improvement, in fact, only beginning after the administration of the salt.—*Bulletin Général de Thérapeutique*, May 30th, 1876.

*On the Value of Phosphate of Lime as a Therapeutical Agent.* By MM. PAQUELIN and JOLLY, of Paris.—The object of this paper is to examine into the origin of phosphate of lime in the system and the mode by which it is eliminated by the urinary and intestinal passages, and, consequently, to estimate the value which this salt may possess as a therapeutical agent. MM. Paquelin and Jolly show that the first portion of the digestive passages exercises no action on phosphate of lime, except to convert it into a superphosphate, and when this latter has reached the intestine and meets the pancreatic

and intestinal fluids it is again precipitated in the form of insoluble phosphate. The authors have made researches which prove that, although the phosphate of lime is condensed in great quantity in the bones, the other organs contain only traces of it, the bile, however, affording a rather large proportion. Phosphate of lime is not capable of absorption, and it has been shown that when given to animals, whether in the soluble or insoluble state, it passes away unchanged in the stools. The authors consider that lime finds its way into the system in the form of carbonate, contained either in the solid food or in the drink, and that it forms phosphate of lime in the system by double exchange with the alkaline phosphates taken also with the food. So, as to the urinary phosphates, they consider them to be formed in the urinary bladder and the biliary phosphates in the gall-bladder. The conclusions they draw are the following:—1. That phosphate of lime is incapable of absorption except in very small quantity. 2. The organism consumes in general very little of this salt. 3. The circulation conveys only insignificant quantities of it, and the tissues, except the bones, contain only some traces. 4. Lime enters the organism in two states, viz., in small quantity as a superphosphate, and in rather a large proportion in non-phosphoric salts. These latter partly exist in the food as carbonate and partly are produced by the decomposition of the alimentary phosphate of lime by the acids of digestion, such as chloride of calcium, lactate of lime, &c. 5. The system forms its phosphate of lime by double decomposition, and finds in the food all the elements necessary to increase, according to necessity, the production of this substance. 6. The phosphate of lime in the urine is in the greater part an intra-vesical formation, and therefore the total amount of the urinary phosphates is not the direct product of disassimilation. 7. The artificial phosphates of lime, soluble or insoluble, are eliminated by the excretory passages without being utilised. 8. The addition of these phosphates to the food is an obstacle to nutrition; and, 9. The soluble preparations of phosphate of lime (superphosphates) act as acid principles.—*Ibid.*, June 15th, 1876.

In the succeeding number of the 'Bulletin' (June 30) the authors of the above-mentioned paper modify their conclusions (7) and (9) in the following sense:—7. Of the two constituents of the phosphates of lime, namely, phosphoric acid and lime, the first is absorbed in a certain proportion in the state of alkaline phosphate, and the second is eliminated directly and almost entirely by the intestinal passages. 9. The soluble preparations of phosphate of lime act primarily as acid principles, and then, by reason of the changes they undergo in the intestines, they act secondarily in a certain degree as phosphatic agents of another base.

*On the Therapeutical Properties of a Spring at Vals, containing Iron, Arsenic, and Phosphoric Acid.*—The spring at Vals named the Dominique contains a peculiar combination of iron, arsenic, and phosphoric acid, and flows from an ochraceous bed in the earth. The iron exists as a basic sulphate; the arsenic as a basic arseniate



of iron, and the phosphoric acid as a basic phosphate of iron. These deposits are formed by the passage and the deposit of the mineral water in the galleries where it is led to the outlet of the natural reservoir formed in the rock. The proportions of the constituents are always the same, and whenever the water is drawn for the preparations of the medicinal articles made from the dried salts, chemical analysis gives the same results. These articles, called "Dragées de Dominique," have been employed with success in intermittent fevers, affections of the respiratory organs, chlorosis and anæmia, and generally in cases of dyspepsia. The arsenic in this preparation is counteracted in its deleterious properties by the iron, and the beneficial effects are further enhanced by the phosphate, and the form in which the preparation is presented is an agreeable one and has a pleasant taste, the *dragées* being a kind of sugar-plum. Various instances are given of the value of this preparation in obstinate dyspepsia, attended with vomiting, and the pleasant taste of the *dragées* enables the patient to crunch them in the mouth like ordinary sugar-plums. It is stated that they never cause constipation, as is usual with ferruginous preparations, while, at the same time, the tonic effect of the iron is increased by the presence of the arsenic. The good effects of an agent so well constituted are said by the editors of the *Union Médicale* to be incontestable, for the iron finds in the other elements, namely, arsenic and phosphorus, adjuvants which act as a vehicle to render it assimilable, which it might not be if taken alone. Arsenic, again, finds in iron a palliative of its poisonous properties, and acts freely as a remedy in intermittent fevers and neuralgia, and phosphorus is also presented as a tonic in this preparation in an advantageous form.—*L'Union Médicale*, August 24th, 1876.

*On Bloodletting as a Therapeutic Agent.* By Dr. C. B. NOTTINGHAM, of Georgia, United States.—In a paper read by Dr. Nottingham, at the annual meeting of the Georgia Medical Association held in Savannah, the author claims for bloodletting an important place among therapeutic agents, notwithstanding the disuse into which it has fallen in recent years. He examines the different reasons which may be urged against bloodletting, and discusses the question of the supposed change of type in disease, which, however, he does not admit. While condemning the indiscriminate and excessive bloodletting which was once practised, Dr. Nottingham still thinks that in certain cases it is a most valuable therapeutic agent, that it often subdues engorgement, abates violent arterial excitement, diminishes preternatural heat, controls pain, and relieves embarrassed respiration. In cases of suffocation and distress dependent on congestion of the lungs or engorgement of the chambers of the right side of the heart, in threatened or existent apoplexy from cerebro-vascular turgescence, in puerperal convulsions from turgescence:—in such cases Dr. Nottingham states that venesection acts instantaneously and beneficially. He thinks that the injurious effects of bleeding are due to the fact that the measure has been sometimes employed at an improper time, and he argues that in inflammations, for instance, it should be resorted to

at the commencement of the disease and not at the period when exudation has taken place, nor should it be used indiscriminately in the feeble and anæmic as well as in the robust, nor in typhoid conditions of the system. Guided by the lights of modern pathology and diagnosis he thinks that blood letting may still be practised with great advantage, and he says that "to the general practitioner a lancet is as necessary in his pocket as any article of the *materia medica*."—*Richmond and Louisville Medical Journal*, April, 1876.

*On the Efficacy of Exutories (Setons, Issues, Actual Cautery, &c.) in the Treatment of certain Cerebral Affections.* By Dr. V. POULET.—Dr. Poulet advocates the utility of revulsives or exutories, especially in the form of cautery, in many internal maladies, and he adduces an instance in which this mode of treatment appears to have been attended with very beneficial results.

The patient was a gentleman in good circumstances, fifty-five years old, who had suffered for several years from a rather extensive eruption of eczema in the popliteal region of one of his legs which had varicose veins. In 1872, however, this eczema had entirely disappeared without any particular treatment, and without the supervention of any appreciable inconvenience, when, on a sudden, symptoms of vertigo developed themselves, together with gastric disturbance in the form of indigestion, and there were also cerebral symptoms of a rather serious character, the memory being lost, the speech often hesitating, the gait uncertain, &c. Remedial treatment was adopted with considerable success, and the patient seemed to be cured, when a relapse occurred, and the attacks of vertigo degenerated into true epileptic fits. It now struck Dr. Poulet that the best treatment would be to place a cautery precisely on the spot of the old varicose eczema, and he accordingly did so with the best effects, for the epileptic fits ceased, the brain resumed its healthy condition, the digestive powers were restored, and, in fact, the patient was completely cured. For the last three years there has been no return of the symptoms, and the patient conducts his affairs as well as he did before his illness. Dr. Poulet, while admitting that probably, in this case, no actual disease of the brain existed, thinks that some organic lesion was imminent, but that it was warded off by the treatment adopted.—*Bulletin Général de Thérapeutique*, June 15th, 1876.

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## REPORT ON PATHOLOGY AND THE PRACTICE OF MEDICINE.

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*Pathology of Cardiac Murmurs.*—In our last report on pathology (April, 1876), we took occasion to introduce to notice the hypothesis advanced on this subject by Dr. Leaming, of St. Luke's Hospital, New York. The terms of the hypothesis were then stated and three cases in proof were briefly narrated. We concluded by noting that he appealed in support of his views to certain experiments made by Dr. Halford, and reported in this Journal in 1860. We have now also to state that he likewise quotes for his purpose some cases recorded by Dr. J. S. Bristowe in the number of this Review for July, 1861. Having proceeded thus far with his essay, Dr. Leaming next presents to his readers the following classification of cardiac murmurs in accordance with their acoustic differences, and then proceeds to further elucidate and apply his hypothesis. The classification is as follows:—

Valvular (all organic)	{ Aortic obstructive systolic. { Aortic regurgitant diastolic. { Mitral regurgitant systolic.
Intra-ventricular (more or less functional)	{ Organic functional. { Inorganic functional.

The sound in valvular murmurs is a friction murmur, that of blood forced through an aperture. The intra-ventricular murmurs are mostly and distinctly chord vibrations. The contraction of the muscular walls of the heart and its fleshy columns, the friction of rushing blood among the chordæ tendinæ and against the tense mitral valve, being the occasion of sound vibrations, but is not the mechanism of the sound itself; for as great difference exists between these murmurs as between that of a whisper and that of the voice. The obstructive systolic aortic may be modified by irregular calcifications in the aortic valves, extending into the column of forced rushing blood. In this way a harsher character may be given to the murmur, or it may even become musical. Vegetation also attached to the orifice or valve may be thrown into vibrations in the column of blood, and produce a musical murmur; but these are rare, mere possibilities. When musical murmurs occur, they are almost always, if not always, vibrations of the chordæ tendinæ, some of which are under extraordinary tension. These sounds or murmurs may be illustrated by a stringed musical instrument. The term "bellows sound" is incorrect, the only friction sound in cardiac murmurs proper occurs where the blood is forced through apertures or past obstructions. The "bellows sound" is heard at the aortic orifice when there is obstruction, as by lymph deposits upon the valve. It is at

first an uncomplicated, simple gushing sound. But in time the obstruction causes hypertrophy of the left ventricle, which, having taken place, irregular tension of the chordæ tendinæ is the result, and vibrations out of unison with the first sound are carried with the current of blood, and both occurring in the systole, are mixed together and form what is called the blowing murmur. It is now a sound of mixed elements, friction of blood against a solid, and vibration of strings under irregular tension. The blowing, sawing, filing, rasping sounds have their origin and cause within the ventricle. They are intra-ventricular. They are heard over the base of the heart, but always with greatest intensity at the apex beat. Friction sounds are heard best over the orifices, or in the direction of the vibrating column of blood. The aortic systolic obstructive murmur is heard over the aortic valves, and in the course of the column of blood. The regurgitant aortic diastolic murmur is heard over the aortic orifice, and to the left and towards the apex beat. The mitral aortic regurgitant is heard behind on the left side near the spine. In this direction the blood is forced in regurgitation through the mitral valve, impinging first against the auricular wall lying against the œsophagus, and aorta, and intervertebral substance; hence the sound is conducted directly into the ear, giving the sensation of being shot into it. It may be heard a short distance from this point, being conveyed through the chest wall. It may be heard in front, at the apex-beat, by conduction through the substance of the heart, when there are no intra-ventricular murmurs to destroy it or take its place. It is one of the most certain of cardiac signs, and an unfailing sign of mitral regurgitation. Its discovery belongs to Dr. Cammann.

A great majority of cardiac murmurs, even of those accompanying organic disease of the heart, are in a manner functional. That is, the murmurs are not organic in the same sense that the valvular murmurs are, which are organic murmurs, because the structural change in the valve is part of the mechanism of the murmur. Intra-ventricular murmurs, even when the result of structural change in the heart, may be considered functional, inasmuch as they have their mechanism in vibrations of the chordæ tendinæ, which are themselves unchanged by any diseased action, but simply vibrate, giving out sound of high or low pitch, soft or harsh, feeble or loud, according to the degree of tension of the individual strings, and the force of the heart's contraction. The cause of the irregular contraction of the heart muscles may be from disturbed nerve power, as well as from organic change.

In concluding his essay, Dr. Leaming notes the fact of cardiac murmurs disappearing when pneumonia or pleurisy supervenes, and attributes the phenomenon to the loss by the thorax of its acoustic properties,—its ability to conduct sound to the ear when applied to it. (Significance of disturbed action and functional murmurs of the heart. By J. R. Leaming, M.D., New York, 1875. *Pamphlet*).

*Amyloid Tumours of the Tongue and Larynx.*—Dr. F. Ziegler narrates a case of multiple tumours at the base of the tongue and of great thickening in the posterior wall of the larynx, which was

encroached upon by a considerable tumour. The case was that of a man, 67 years of age, who was brought into the hospital moribund, and nothing was known of the tumours until they were revealed on examination after death. There were three much larger than the rest; one on the right side and two on the left; the latter pressed against the epiglottis. All the tumours were covered by the mucous membrane which, in the case of the smaller, was unchanged, but in the large was glazy and thin. In form they were hemispherical or oval, and in consistence were of a woody hardness. The diseased process was found to be gradually extending into the surrounding sub-mucous tissue and the subjacent muscles, (the hyoglossus and the genio-hyoglossus, the fibres of the latter losing themselves gradually in the morbid mass.

In the larynx the deposit on the cricoid cartilage posteriorly had acquired a great thickness, and was prolonged in the direction of the ligaments. In consistence it resembled the substance of the lingual growths and like them had its seat in the soft tissues beneath the mucous membrane. It did not affect the cartilage itself. It lessened in thickness at its upper border, and did not extend to the epiglottis. Besides this growth on the cartilage, there were also two pediculated polypoid growths, situated above the false vocal cords, each having a dense nuclear portion. Below the false cords the larynx was free from disease and the muscles intact.

There was hypertrophy and dilatation of the heart, the tissue of which was fatty; the lungs were emphysematous, with hypostatic congestion, cedema, and bronchitis. The spleen large and soft. The kidneys normal: very large hydrocele on both sides. The liver exhibited central red atrophy in a slight degree, and on the anterior border of its right lobe were two deep radiating contractions, about which the serous capsule was thickened. When cut into the normal hepatic tissue was seen to be replaced by dense connective tissue dotted with yellow points. These hepatic lesions were at once assigned to syphilis, and suggested to Dr. Ziegler that the lingual tumours had a like origin, and were to be regarded as syphilitic gummata.

The application of chemical reagents both without and with the aid of the microscope, most distinctly indicated the amyloid composition of their morbid matter. Iodine coloured it immediately a deep blue, a bright green or a violet or brownish red colour, the blue exhibiting itself especially in the centre. Under the microscope the surrounding tissues were seen to be gradually invaded by the morbid deposit. The sarcolemma of the muscles, as it were, withered before it; it seemed to project into it by pointed ends, surrounded by a hyaline ring, progressively thickening. The amyloid substance encroached also on the sub-mucous tissue and on the mucous membrane, and presented a special affinity for the walls of the arteries, and, but in a very much less degree, of the veins likewise. The nuclei of the cells of the connective tissue were the last to disappear in its transformation; and the fatty tissue offered great resistance to its advance. In the course of their change into amyloid matter the fat cells lose their form, and patches of the deposit make their ap-

pearance in them and by progressive growth ultimately entirely supersede them. The salivary glands first suffered in their tunica propria and in the end became entirely amyloid, their epithelial cells holding out longest against the process. The follicles of the tongue had generally escaped, except in the mucous membrane covering the largest growths. The nerves showed unusual resistance and were often visible in tissue that had become transformed. Ziegler concurs with Billroth in holding that inflammation in a tissue favours the amyloid change, and he conceives that the morbid process going forward in syphilitic gummata is of an inflammatory nature and consequently favourable to the deposit of the morbid albuminoid material. In the cicatricial tissue of the liver, in the case in question, amyloid matter infiltrated the vessels; and an examination of gummatus tumours, from other subjects, showed Ziegler that their tissue and especially their blood vessels were the seat of amyloid change. His general conclusion is that the process is one of infiltration. (*Virchow's Archiv*, December, 1875, p. 273.)

*Lymphadenoma without Leukæmia.*—A case of this sort occurred at "l'hôpital de la Pitié" under the care of M. Desnos. The patient, a man, æt. 34, had enjoyed good health; had never suffered with fever, and not been out of France. He had, however, on two occasions, a bubo following on a simple chancre. No hereditary antecedent either tubercular, scrofulous, or cancerous. Nine months before admission he had severe lumbar pain; latterly this became very acute, his general health suffered greatly, with want of appetite, vomiting, and slight diarrhoea; and a week or more before admission he noticed an enlarged gland in his right groin. In this condition he was subjected to large doses of iodide of potassium, which produced a molecular eruption and other signs of iodism; and superadded to all this there was the occurrence of jaundice. When admitted he was deeply jaundiced, very cachectic in appearance, and much wasted and extremely weak. Anorexia, bilious vomiting, and constipation. Nothing abnormal noted in heart and lungs. In the right groin there was a gland enlarged to the size of a chicken's egg; but the skin covering it was natural in appearance. Another large tumour, the size of a man's fist, occupied the posterior triangle of the right side of the neck; also, two enlarged glands existed in the right axilla. After two days similar growths appeared in the left axilla, and almost immediately afterwards, a large tumour sprang up on the left side of the neck, together with enlarged glands in the groin on the same side.

The jaundice was attributed to neoplastic lymphatic growths in the pelvis of the liver, compressing the ducts, or to lymphadenoma within the hepatic tissue itself. The liver was, indeed, considerably enlarged, and rather tender on pressure; but its surface when felt, was smooth and even. The spleen was likewise much enlarged. During his residence in the hospital, extending from the fourth of September to the eighteenth of November, when death supervened, the patient was the victim of great pain, chiefly in the loins and thighs; suffered from vomiting, constipation, wasting, and sleeplessness. Large tu-

mours formed in the muscles of the thighs, and others under the jaw, and elsewhere. Towards the end œdema appeared in the legs, and mounted to the thighs and scrotum; the abdomen was very tympanitic, but no signs of ascites were discoverable. The urine was free from albumen and sugar; the senses and intelligence remained intact. On the fifth of November, the jaundice is noted to have disappeared, and the skin, except over œdematous parts, to have assumed a leaden hue.

On several occasions the blood was examined, but this never showed an increase in the number of white corpuscles, although the globules were counted.

The autopsy was made twenty-four hours after death. There was slight pleuritic effusion on the right side; the lungs normal, as also the thyroid. Two bronchial glands were enlarged to the size of a nut. The volume of the heart was normal; its valves and muscular fibre healthy; but a little spherical ganglion was seen on the visceral surface of the pericardium, of a hard consistence and white, to the left of the apex. Also three or four similar but smaller growths existed on the posterior aspect, near the auriculo-ventricular furrow; and a much larger one within the substance of the posterior wall of the right auricle, obtruding somewhat on its inner surface. At the level of the inter-ventricular sulcus were some white crossing slender lines, side by side with the coronary vessels, which could be followed to the apex, and were evidently lymphatic vessels. Whitish streaks also existed on the diaphragm, but more abundantly on its concave aspects. In the course of some of these were greyish specks. On opening the abdomen the whole of the intestines were hidden by the great epiploon. This was of great thickness, as much as three centimètres in places, and infiltrated by myriads of small ganglionic tumours, some as large as haricots. The liver much enlarged, weighing 1800 grammes. Over its two surfaces were diffused numerous small points, some assuming the form of little tumours of the size of a lentil and penetrating into the hepatic substance. Similar tumours showed themselves, on section, throughout the parenchyma.

The gastro-hepatic epiploon and the blood and biliary vessels in the hilum of the liver, were lost in an irregular mass of the size of a child's head at full time, and very adherent to the liver. It consisted of a greyish hard tissue, and rather resembled encephaloid. This mass, the pressure of which explained the jaundice, extended behind the stomach, and was there joined to a still larger mass situated along the spine. The spleen had attained an enormous size, being fifteen centimètres long and eleven broad; and weighed 440 grammes. Its tissue was red, rather soft, and had scattered through it a great number of small white lenticular masses; one such mass was as large as a two-franc piece. The supra-renal capsules were normal, as was likewise the left kidney, except that only two small greyish nodules were noticed, like those found in the liver. On the contrary, the right kidney was completely disorganised, its normal structure being replaced by an irregular mass of dense greyish tissue, with here and there some soft yellowish spots. At a few places small isolated remnants of cortical substance could be seen. The pancreas and

stomach exhibited no morbid features. The mesentery was thickened, vascular, and occupied with numerous small granular tumours. The mesenteric glands were of huge size; two of them as large each as an orange, of a dull white appearance on section, with some yellowish points near the centre. The intestines were highly vascular, but neither their peritoneal nor mucous coat showed signs of disease. However, the visceral layer of the peritoneum presented some small patches of purulent exudation in a few folds of the small intestines; and the parietal layer at places, exhibited white lines like those on diaphragm.

On pushing aside the mass of intestines, an enormous lobulated tumour, of hard consistence, presented itself to view. It was of the size of an adult head, and occupied the median and lateral portions of the dorso-lumbar region, between the eleventh dorsal vertebra and the posterior surface of the stomach, where it was in union with the large post-gastric tumour already spoken of. It also projected forwards and downwards into the pelvic cavity, and there became continuous with enlarged pelvic glands. It adhered firmly to the spinal column, but admitted of being dissected therefrom, leaving intact its structure. From its position it compressed the lumbar plexus, and the vena cava inferior, together with the aorta, a part of the sacral plexus and the rectum. This condition of things affords a ready explanation of the severe pain in the loins and lower extremities, and likewise of the constipation and oedema. The abdominal aorta coursed through the tumour in a channel which appeared to be hollowed out for it. Its calibre, consequently, remained unaffected. This large tumour had the usual structure of lymphatic glandular tumours. The lymphatic system generally, in short, was affected. The enormously developed glands in the neck did not involve the air-passages and blood vessels, nor the pneumo-gastric nerves. On section they had a greyish white colour, and looked like encephaloid, exuding little fluid under the knife. Some little islets, of softer consistence and a yellowish colour, were visible about the centre of the morbid mass. Similar formations occurred in the great pectoral muscles, in the arm-pits, and groins; smaller and fewer, however, on the left side of the body than on the right. Lastly, a large mass had grown on the right thigh, in the vastus externus, and bound down by the fascia lata, but without any capsule of its own. Nor had the skin escaped, for in that covering the thorax and the right thigh, where it had a pale violet colour, four or five small tumours were connected with it.

The encephalon exhibited no trace of disease. A microscopic examination of prepared portions of the tumours showed the same structure to be present in all, wherever derived. This structure consisted of a reticulated tissue, enclosing within its meshes a large number of lymphatic corpuscles, granular and irregular in shape.

Thus every portion of the history and post-mortem examination pointed to the case as one of lymph adenoma. Nevertheless, in its early history, the diagnosis of the case was not unencumbered by doubt; other chronic cachetic conditions offered themselves in expla-



nation, viz. results of marsh-poison, tuberculosis, syphilis, scrofula, and cancer. The first named was excluded on account of the constant residence of the patient in a healthy country, and the absence of intermittent paroxysms. So, on the other hand, the cachexy of marsh fever is never associated with so general hypertrophy of the lymphatic system. The second, tuberculosis, was as readily set aside, for there was no cough, dyspnoea, hæmoptysis, or hereditary predisposition; and the persistent hardness of the tumours was opposed to the supposition of tubercle. Scrofula was equally eliminated by the absence of its special signs; whilst, contrary to its pathological history, the glands here showed no tendency to softening or suppuration. Syphilis again was excluded by reason of the absence of its antecedents in the case. Lastly, cancer was negatived by the absence of hæmorrhage and ulceration, and also of the character of the tumours and their non-adhesion to the skin. Lastly, the enumeration of the globules of blood under the microscope proved the red corpuscles to be normal, and the white cells to be scarcely modified; whereas, in cancer, the hæmatin undergoes considerable reduction.

Consequently the diagnosis of the case as one of lymph-adenoma, or lymphadenia, was affirmed. From a review of the history of this case it would appear that the disease started with the morbid mass, ultimately so enormously developed, in the lumbar region; and thence was propagated along the vessels to the groin, where the first tumour was noticed. In its deeper course it attacked the mesentery, advanced to the pelvis of the liver, the spleen, and kidneys; and, making its way by the lymphatics of the diaphragm, invaded the thorax, attacking the heart, the bronchial and cervical glands. The lungs in this case escaped, as they usually do. Although the kidneys were affected, and one of them completely disorganised, there, nevertheless, was never a trace of albumen in the urine. M. Vidal has noticed this fact in like cases, and remarks that where albumen appears, it may be assumed that the case is complicated by Bright's disease. The only changes observed in the composition of the urine were an increase of water and uric acid, and a diminution of phosphates, sulphates, and chlorides. Like the lungs the brain also escaped, and no cerebral symptoms occurred during life. On the contrary, in leukæmia disturbances of vision are usual, with retinitis, ecchymotic spots and milky patches on the optic nerve, with dilated gorged vessels.

The notes of this case are accompanied by a history of the disease from the time Dr. Hodgkin pointed it out as a peculiar pathological state. By several writers it has been considered to be essentially associated with leucocythemia, and Wunderlich supposed it to be a prelude to this latter morbid state, but it is now clearly shown, by the preceding and other cases, that leucocythemia is only an accessory condition, and may be present or absent. Consequently M. Desnos would detach its history from that of leukæmia, and class it with other parallel states under the term "lymphogenic diathesis," as proposed by Jaccoud.—*Gazette Médicale de Paris*, August, 1876.

*Symptomatic Hepatic Fever from Occlusion of Hepatic Duct.*—The following remarks on this subject are abstracted from a course of lectures which M. Charcot has recently delivered at the Faculté de Médecine of Paris, on diseases of the liver, and which are characterised by the same power of observation and analysis so distinctive of the teaching of this eminent physician.

As a consequence of obstruction of the common duct the biliary ducts in the substance of the liver become greatly dilated, and after a while suffer changes, of greater or less severity, in their walls and contents. The interlobular canals partake comparatively very little in the dilatation.

The change usually discoverable within the principal biliary passages consists in the disappearance of their cylindrical epithelium, a circumstance not found in the interlobular canals. Mostly the dilated ducts contain a viscous bile, mixed with mucous flakes and débris of columnar epithelium, and, sooner or later, biliary sand. But it happens occasionally that, while the small biliary ducts in the hepatic substance are charged with bile, the large biliary canals are filled with a mucous liquid, destitute of the least trace of pigment or of biliary acids. As a rarer condition, the ducts contain a mucopurulent fluid, and in such case the lesion may be described as one of suppurative angiocholitis.

The inflammatory lesions, however, are not confined to the ducts, but commonly extend, as a hyperplasia, to the capsule of Glisson. Now and then, they give rise to the local formation of pus in the same structure, with the consequent formation either of a large abscess or of numerous disseminated lenticular abscesses.

Another result of the obstruction of the common duct, arising from pressure of indurated tissue or of the distended biliary ducts, is arrest of the intra-hepatic circulation and stasis in the vena porta, with consequent ascites, hypertrophy of the spleen, and gastric and intestinal hæmorrhage. In some cases of gastro-intestinal hæmorrhage the bleeding proceeds from ulcers of the mucous membrane of the stomach. In other instances of hæmorrhage, such as that from the nose, or from leech-bites, the bleeding appears referable to alterations of the blood or of the blood-vessels; and this alteration has been attributed to the solvent action of the biliary acids retained in the blood. M. Charcot, however, quotes some experiments of Vulpian to show that this explanation is inadmissible, inasmuch as the quantity of such acids when retained cannot be sufficient to produce the effects referred to. The same objection holds good to the attributing to this same cause the weakened action of heart with its results, the formation of clots in the auricle and hæmoptoe infarctus in the lungs, and the occasional nervous accidents terminating life in the form of convulsions and coma.

But there is another symptomatic condition, known as "symptomatic" intermittent fever, which occurs at times, without any trace of actual hepatic colic, upon calculous obliteration of the common duct, and upon intra-hepatic biliary lithiasis, and, in fact, as a consequence of occlusion of the biliary duct from any cause;

as, for instance, fibrous contraction, or pressure upon it of cancer in the head of the pancreas.

The anatomical condition most favorable to the outbreak of this fever appears to be the presence of muco-pus mixed with stagnant bile in the biliary passages. It is, nevertheless true, that suppurative angiocholitis may exist without the occurrence of this intermittent fever; and, on the other hand, that this fever may arise when, rightly speaking, no suppuration is found in the biliary ducts. And it is equally possible that secondary hepatic abscesses may be wanting when this fever is present. To account for such ambiguous phenomena, M. Charcot concludes that there must be present, in the dilated and inflamed ducts, a septic principle, or "pyretogenic" poison, the product of changes occurring within the biliary liquid itself.

But whatever be the exciting agent, an analysis of twenty cases shows that neither jaundice nor hepatic colic, although not infrequent, are not its ordinary concomitants. The following are its phenomena:—1. The onset is sudden, commencing with a rigor, followed by heat and sweating, just as in an ordinary case of intermittent fever. Of the three stages the sweating is most prone to fail. 2. The non-febrile periods are very frequently clearly marked, and the accessions regular in their occurrence, simulating the quotidian, the tertian, or the quartan type. But to this rule many exceptions obtain. 3. M. Regnard has laid it down, though only from a single case observed, that it is a characteristic sign of this hepatic fever, distinguishing it from true fever, that the specific gravity of the urine is reduced, and that leucine and tyrosine are present. M. Charcot, however, considers that the reduction in the proportion of urea is no special feature of the fever, but a result of merely damaged hepatic function. 4. As happens with symptomatic fevers generally, the accessions of hepatic fever take place in the evening, instead of the morning, as is the rule in idiopathic fever. 5. Hepatic fever is for the most part chronic. For instance, it may endure two or three months, with intervals, in which no accessions occur, of eight, ten or fifteen days. 6. A favorable issue is possible—M. Hensch has reported one instance.

This hepatic intermittent fever is separable from a form of fever coming on at times in the course of hepatic colic. Both alike are probably due to a similar pathogenetic cause—a septic matter derived from altered bile. The hepatalgic fever may be presumed to be set up from the passage of the gall-stone, which may either lacerate the mucous membrane of the biliary duct, or otherwise so increase the pressure in its interior as to facilitate absorption of the septic material. Or, again, the passage of the calculus may set up acute inflammation, the products of which may mingle with the bile and bring about, after the manner of a ferment, a very rapid alteration of its constitution. In support of this hypothesis it may be urged that the hepatalgic rigors scarcely ever happen except in case of patients a long time exposed to inflammatory lesion of the biliary duct: a condition of things favorable to the formation of a morbid material.

Further, the rigor is sometimes the prelude to a series of febrile paroxysms, of greater or less regularity, but not without the recurrence of hepatic colic; and this phenomenon supports the second clause of the hypothesis. Of the intermissions of hepatic fever no explanation is at hand.

In the same lecture, M. Charcot proceeds to point out the intimate analogy between the phenomena of hepatic fever and those of "urethral" or "uro-septic" fever, as seen in persons suffering with retention of urine consequent upon bladder and prostatic disease, and he takes occasion to describe the morbid renal condition known as "surgical kidney."—*Le Progrès Médical*, August, 1876.

*Alteration of the Peripheral Extremities of Cutaneous Nerves in Pemphigus.*—M. Déjérine presented to the Academy of Sciences of Paris notes of a case of pemphigus, in which the cutaneous nerves were microscopically examined. He prefaced his observations by some general reflections on trophic changes of the skin dependent on nerve lesions, noticing among others ulcerations, bullæ and morbid smoothness of the skin. It is always, therefore, necessary to remember that skin diseases may not be spontaneous and independent morbid states, but be referable to alterations of nerves and nerve centres. In the instance of herpes zoster this relation has been demonstrated by Charcot, Barrensprung, and others.

M. Déjérine's case was that of a woman admitted into the St. Louis Hospital, Paris, suffering with general paralysis and rhythmical tremors of the upper and lower extremities; appearing only, however, when voluntary movements were attempted. The woman died a month after admission. Ten or twelve days before death an eruption of numerous bullæ (about twenty in all) appeared over the arms and legs, chiefly over the extensor muscles. In size they were from two to three centimètres in diameter; contained a liquid of a lemon tint, similar to that found in vesications from a blister. The examination of the sensibility of the diseased spots gave no valid results, the patient's prostration being too great to enable her to reply to questions.

The autopsy displayed diffused meningo-encephalic lesions; and examination of the spinal cord, after hardening in a weak solution of chromic acid, revealed the existence of bilateral and symmetrical sclerosis of the lateral columns, extending the whole length of the cord. At the same time the grey matter and the posterior columns were quite healthy.

A portion of skin with subjacent areolar tissue was removed for examination, from the site of the bullæ. The portions so removed were placed for twenty-four hours in an aqueous solution of osmic acid (one part to 500 water), next washed in distilled water and then immersed in a solution of picrocarminate of ammonia. From portions so treated, and when washed in distilled water, numerous microscopic sections were prepared. On examination the nerve tubes present were for the most part found altered in structure. They had assumed a moniliform appearance, due to

breaking up of the myaline, which occurred in blackish drops, occupying at intervals the envelope of Schwann. The intermediate spaces within the envelope were filled with a protoplasmic, yellowish material. The nuclei of the sheath were probably rather augmented in number, but not decidedly so. No trace of the axis cylinder was discoverable in the altered tubes. In portions of skin from between the bullæ these altered nerve-fibres were few in number. In conclusion, M. Déjerine surmises that the alteration of the nerve-fibres extended from these peripheral ends to the trophic centres, whereon the nutrition as well of the cutaneous nerves as of the skin itself is dependent.—*Gazette Médicale de Paris*, August, 1876, p. 408.

*Rheumatismal Myitis.*—This subject is treated by Dr. Uno Halleday, of Stockholm, in the 'Nordiskt Medicinskt Arkiv.' The author regrets the small amount of attention bestowed on the pathological conditions of inflamed muscular tissue.

In old rheumatic cases he finds an excessive production of connective tissue with corresponding atrophy of the muscular elements. The pain of the malady usurps the chief attention both of patient and doctor, but it is not a reliable symptom, and commonly not well defined. It varies considerably in character in the same and in different individuals, and may from time to time disappear. It does not always correspond with the part really attacked, and may occasionally be absent. Moreover, similar pain accompanies many other maladies. It is not uncommon to meet with pain seated a considerable distance from the seat of the myitis, due in all probability to pressure exercised by the swollen muscular tissue upon nerve filaments, at the peripheral extremities of which pain will manifest itself.

Increased sensibility over the affected part is not a symptom of importance, as the same thing happens in many other maladies where there is no myitis. The function of the affected muscle is generally impeded; but such a condition owns other causes, and groups of muscles apparently the subject of paresis may present no traces of inflammation. The source of the condition is far removed from the local trouble. External swelling of the affected muscles is mostly very slight or wanting. Palpation, carefully used, is the best aid to diagnosis. The characteristic features of myitis depend largely upon the consistence of the tissue felt, but above all on its muscular elasticity. This last resembles that of muscle in a state of contraction; and, indeed, at the diseased spots, the tissue may offer the hardness of a board on pressure, exceeding the induration caused by normal contraction, although in the lesion in question a state of contraction is unknown. It is usually the case that the inflammation exhibits itself in detached portions or areas, and these hardened sections are difficult of movement one towards the other. Nevertheless induration is not an essential phenomenon; in some instances the consistence is normal or even diminished, but always the default of elasticity is constant. This last feature is a pathognomonic sign; but its discovery and appre-

ciation require patience, a close examination, and a previous acquaintance with the characters of normal elasticity in muscle.

Muscles are seldom affected throughout. The most common seat of inflammation is in the vicinity of their attachments. At times a number of nodosities may be found disseminated in one or more muscles, the intermediate muscular mass being healthy. The more advanced the anatomical changes, the less do patients in general complain of sensibility to touch.

Treatment should be directed to the removal of the morbid product, and therefore the employment of means to promote its absorption. *Massage* is the best means to this end, because it acts directly on the parts affected, and also possesses a more certain influence on the absorbent system than any other remedial means, whether external or internal. In recent cases its effects are frequently marvellous; but even in very chronic cases it will do good service, although the destruction of muscular tissue forbid the hope of cure. But this remedial measure must be continued not only until pain ceases and the patient feels himself recovered, but onwards and afterwards until every trace of the disease has vanished. In dealing, indeed, with bad and chronic cases the time occupied must not be heeded.

The details of eight cases are given, illustrating the rules laid down as to diagnosis and the great curative powers of the plan of *massage*.  
—*Nordiskt Medicinskt Arkiv*, vol. viii, part ii.

## REPORT ON MIDWIFERY, DISEASES OF WOMEN, AND DISEASES OF CHILDREN.

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### MIDWIFERY.

1. *The Hereditary Transmission of Syphilis.* By Dr. KASSOWITZ ('Stricker's Jahrbucher,' Part iv, 1876).
2. *On the Prophylaxis of Puerperal Fever.* By Prof. BISCHOFF ('Allg. Med. Cent. Zeit.,' Feb., 1876).
3. *Cases of Puerperal Fever with reference to Epidemic Origin.* By Dr. BRUCE ('Edin. Med. Journ.,' July, 1876).
4. *A case of Primary Abdominal (Extra-Uterine) Pregnancy.* By Dr. SCHMITT ('Memorabilien,' 1874).
5. *Three cases of Stone in the Bladder complicating Pregnancy.* By Dr. HUGENBERGER ('St. Petersburger Med. Zeitsch.,' vol. v, 1875).
6. *Embolism of the Pulmonary Artery, following the application of Eschsch's bandage to the lower limbs.* By Dr. MASSARI ('Wien. Med. Woch.,' No. 48, 1875).
7. *On Syphilitic Manifestations in Pregnant and newly delivered Women.* By Dr. MORET ('Thèse de Paris,' 1875, Delahaye).
8. *The Originator of the Double-curved Midwifery Forceps.* By Dr. MCCLINTOCK ('Dublin Obstetrical Society,' April 8th, 1876).
9. *Fibro-Myoma in the Vesico-Vaginal space obstructing Delivery; spontaneous expulsion; artificial enucleation; recovery.* By Dr. EDUARDO PERRO ('Ann. de Gynec.,' Jan., 1876).
10. *Dropsy of the Amnion—Twin Pregnancy.* By Dr. LE ROY de Langevinière ('L'Année Médicale,' July, 1876).

1. Dr. Kassowitz gives the results of his observations at the Vienna Childrens' Hospital, where he had unusual opportunities for investigation the subject of the hereditary transmission of syphilis.

He found that infection by the sperm-cell or germ-cell, or both, was the common method.

A healthy mother might bear a syphilitic child without herself becoming infected; and further, a mother who became infected with syphilis after conception of a healthy child could not infect the child.

It is stated, however, that the mother's syphilis may cause abortion, although the ovum is not syphilised. Syphilis, in fact, is said to be transmissible to the offspring by either parent only at the time of conception.

It would seem that the syphilitic virus differs from that of small-pox, scarlet fever, &c., inasmuch as it is incapable of transmission through the placental tissues, *i. e.*, the syphilitised fœtus cannot infect a healthy mother, nor a syphilitised mother a healthy fœtus through this channel.

[There seems to be evidence from the pathology of the nervous structures that mothers who bear syphilitic children without themselves displaying the ordinary phenomena of syphilis are liable to cerebral and spinal lesions at periods long remote from the time of the infecting pregnancy. I believe that the experience of Dr. Broadbent substantiates this.—*Rep.*]

2. Believing that puerperal fever, in its manifold forms, arises only from infection through wounds of the genital canal, chiefly carried into the parts from without, though occasionally autogenetic, Professor Bischoff strongly recommends the free use of carbolic acid during all labours; baths and injections containing it being given, and the hands and all instruments are washed in a three per cent. solution of the acid, or a preparation containing ten per cent. in oil or glycerine.

All lacerations should be united, where practicable, by carbolised silk, or by silver wire. After labour, injections of a two per cent. solution of carbolic acid should be used, and if a catheter is used it should be anointed with carbolised oil. Statistics are given, showing a greatly lessened rate of mortality since the use of carbolic acid.

3. Dr. Bruce relates his experience of puerperal fever, so called, for a period of twenty-one years, during which time he attended 3500 cases, of which twenty-two presented symptoms referable to puerperal fever. Small-pox and scarlet fever accounted for six, and of the rest the causes were various, the majority being grouped under the heading "unknown."

Dr. Bruce states that his experience entirely accords with that of Dr. Matthews Duncan, who, at the beginning of this year, read a paper before the Obstetrical Society of Edinburgh to disprove the assertion that occasional outbreaks of puerperal fever occurred in an epidemic form.

4. In this case of extra-uterine gestation the fœtus was developed within the peritoneal cavity and the pregnancy had passed the term by a month according to the patient's belief.

Nothing abnormal had been observed during the pregnancy, but the day Dr. Schmitt saw the patient she complained of severe pains of an expulsive character. Some hours later the fœtus was living, as was proved by the existence of fœtal heart sounds. While Dr. Schmitt was examining the patient a bag which projected considerably into the vagina suddenly burst and gave exit to a quantity of fœtid fluid. With this came the right arm of the child. Turning having been found to be impossible, even under chloroform, the patient died undelivered during convulsions.

At the autopsy the fœtus was found to be without envelopes; the placenta was attached to the mesentery; all the organs were normal except at the seat of rupture in the vagina.



5. Dr. Hugenberger gives the particulars of three very interesting cases of stone in the female bladder, all occurring during pregnancy, and two complicating delivery.

In the first case delivery was ultimately accomplished by means of the forceps without the removal of the stone, and the patient recovered. In the second case labour pains were excited prematurely, and on examination a stone was found in the bladder and was removed by vaginal lithotomy. After this the labour pains went off for fifteen days, when they came on again, and the patient was delivered in about the thirty-fourth week of her pregnancy. Rigors and fever set in from this time and she died with symptoms of peritonitis. Chronic interstitial nephritis, pyelitis and purulent collections around the uterus were found.

The third case was that of a primipara who had had retention of urine ever since her labour set in. On examination a stone was found to obstruct the descent of the head and also the passage of urine. Vaginal lithotomy was performed and an hour afterwards delivery occurred spontaneously. Three days subsequently the patient died of septic peritonitis. Post-mortem; the kidneys were found to be enormously enlarged and soft. There was also pyelitis and chronic cystitis.

6. A patient who had had ten deliveries without accidents was brought to Professor Spaeth's clinique in the sixth month of her eleventh pregnancy on account of uterine hæmorrhage. The patient was so anæmic that labour was induced by Braun's colpeurynter and completed by turning. Though there was but little hæmorrhage symptoms of collapse came on. For this the lower limbs were enveloped in Esmarch's bandage from the great toes up to the upper third of the thighs. After this the pulse returned, dyspnœa diminished and the general state improved so that the patient took nourishment.

Three times in the course of two days the bandages became so painful that it was necessary to undo them, but they were re-applied on account of the recurrence of the symptoms of depression. On the following day as the bandage was being removed from the left leg the patient fell over, the face became pale, the extremities cold and the pulse imperceptible. The agony lasted two hours, during which there was irregularity of the heart, extreme dyspnœa, and ultimately stertorous breathing. Post-mortem, several clots were found adherent to the internal tunic of, and likewise blocking, branches of the third order of the pulmonary artery. Similar clots filled the saphenous vein and its branches in both limbs.

7. Dr. Moret's thesis is divided into three parts. In the first he has studied the influence of pregnancy on the appearance of syphilitic manifestations, and he finds it is often the occasion of them; in support of which he adduces a number of observations showing that each pregnancy in certain women affected with syphilis was attended by syphilitic eruptions on the skin or mucous membranes.

In the second part Dr. Moret studies the grave influence of pregnancy on syphilis. In the pregnant woman the syphilitic fever

is longer and more intense; the eruptions of mucous tubercle on the genital organs are more confluent. The same statements hold good with regard to the various syphilides which may be seated about the anus, vulva, and perinæum. Syphilitic rheumatism is more acute, and is accompanied by more pain and by more intense fever. Tertiary symptoms come on early and anticipate the secondary accidents. The third part of the thesis is devoted to treatment, but there is nothing specially calling for comment, as the treatment is the same whether the patient be pregnant or no.

8. Dr. McClintock has lately inquired into the history of the double curved forceps, with the purpose of ascertaining to whom the credit of inventing the second, or pelvic, curve belongs. His learned investigations show that the first person who devised and made use of this form of forceps was Dr. Benjamin Pugh, of Chelmsford, Essex, who had the instrument made about the year 1736. But both Levret and Smellie also had a similar instrument in use about the same time—Levret in 1747 and Smellie in 1751. It is possible that the idea of a pelvic curve occurred to each of those eminent men independently, for there is no evidence to show that they were acquainted with the previous discovery of the instrument.

9. This is a remarkable case. The patient, a primipara, was in the Maternity Hospital at Milan. When she fell in labour it was found that although the pelvis was normal, there was a mass in the anterior part of the vagina which descended before the foetal head. The patient's condition being favorable it was decided to wait, and it was found that the mass descended so as ultimately to come below the vulva, and it then became fixed below the pubic arch. The foetal head soon followed, but it was found that when fully born the child was dead.

Nothing was done to the mass until the third day, when the vaginal wall covering it was found to be getting into a gangrenous state. Incisions were then made, and the mass was enucleated without any pedicle being found. The growth was supposed to have been originally sub-peritoneal and behind the bladder. The patient made a good recovery.

10. The patient, a multipara, supposed herself to be six or seven months pregnant, but she suddenly began to have rapid enlargement of the abdomen, and at the end of seven months she was larger than if at full term. The abdomen was very tense; fluctuation was marked; there were some uterine contractions however, and foetal movements and heart sounds were observed. Labour set in with escape of water two months before the expected time, and before the os was dilated, so that the accoucheur prevented the sudden and complete escape of the fluid by plugging the os with his finger. In this way he controlled the labour until the contractions became regular. The patient recovered.

[A very similar case has lately occurred in my practice.—*Rep.*]

## DISEASES OF WOMEN.

1. *On Incision and Dis-cission of the Cervix-uteri.* By Dr. E. R. PEASLEE ('Amer. Journ. of Obstetrics,' August, 1876).
2. *Vaginal Hernia, or Enterocœle.* By Dr. FORDYCE BARKER ('Amer. Journ. of Obst.,' June, 1876).
3. *Formation of an Artificial Ureter for Urinary Fistula arising after Ovariectomy.* By Prof. VON NUSSBAUM ('Edin. Med. Journal,' July, 1876).
4. *On the Pathology of Membranous Dysmenorrhœa.* By Dr. BEIGEL ('Arch. f. Gynæk.,' Band ix, heft i, 1876).
5. *On the differential diagnosis of Sub-peritoneal Serous Cysts and Ovarian Cysts.* By Dr. KÖBERLÉ ('Gaz. Méd. de Strasbourg,' No. 1, 1876).
6. *Retention of Menses in one half of a Double Genital Canal (Hæmato-colpometra Laterale).* By Dr. FABBRI ('Lo Sperimentale,' Feb., 1876).
7. *Spontaneous Rupture of an Ovarian Cyst.* By Dr. KRYZAN ('Centr. f. Chir.,' No. 1, 1876).
8. *Carcinoma of the Ovary, Umbilicus, and Sternum.* By Dr. KÜSTER ('Beiträge zur Geburts. und Gynæk.,' Band vi, heft 1, 1875).
9. *Cancerous degeneration of both Ovaries during Pregnancy.* By Dr. HEMPEL ('Arch. f. Gynæk.,' Band vii, heft 3, 1875).
10. *Amputation of the body of the Uterus in a case of inversion; recovery.* By Dr. PALASCIANO ('Arch. di Chir. Pr.,' vol. xii).
11. *Remarks on the Enucleation of Uterine Fibroids.* By Dr. GAILLARD THOMAS ('American Archives of Clin. Surg.,' July, 1876).

1. Dr. Peaslee read an elaborate and able paper on this subject before the New York Academy of Medicine on June the first of the present year.

Dr. Peaslee comments severely on the operations proposed and practised by the late Sir James Simpson and by Dr. Marion Sims, and points out their dangers, viz. hæmorrhage, pelvic cellulitis, septic peritonitis, abortion, and sterility; such dangers arising mainly from the extent of the incisions.

Admitting that enlargement of the calibre of the internal os is indicated in certain cases of dysmenorrhœa, &c., Dr. Peaslee advises an operation which greatly limits the extent of the incision. This small operation is preferable for certain reasons to the slow and possibly painful method of dilatation.

The operation recommended by Dr. Peaslee should not be undertaken if a sound, one eighth of an inch in diameter or more, could be passed through the internal os; the obstruction arising from stenosis and not from flexion. If the external os would only admit a sound one sixth of an inch in diameter stenosis as regards conception probably existed and an operation might become necessary.

The operation recommended by Dr. Peaslee consisted chiefly in making a slight incision extending partially or completely through the mucous membrane, according to the requirements of the case, the object being to obtain a passage equal in calibre to that of the normally formed canal. The operation should be performed about a week before or after a catamenial period. The patient should be kept in bed for two or three days and not be allowed to walk for a week. Dr. Peaslee has done the operation 300 times and has never known peritonitis or pelvic cellulitis follow except in three cases, and in those but slightly.

Discussion ensued upon the reading of Dr. Peaslee's paper, in which Drs. Fordyce Barker, Emmett and Pallen took part. Dr. Fordyce Barker said that it was high time the matter was discussed, for there existed a belief that the leading physicians of New York frequently resorted to the operation for the cure of all sorts of women's illnesses. The operation was doubtless required in certain cases, but not in so large a number as some believed. He concurred in some of the remarks made by Dr. Peaslee.

Dr. Emmett agreed with Dr. Barker and the author of the paper; while Dr. Pallen dissented.

2. In an able Essay on the subject of vaginal hernia, Dr. Fordyce Barker makes some valuable statements. Usually the hernial protrusion is behind and to one side of the uterus, more rarely in front. The bowel which most frequently protrudes is the ileum; but in some cases the colon and the cæcum have been involved.

The accident is commonly attributable to physical effort; but occasionally no cause can be assigned. The general symptoms of acute vaginal hernia are those of hernial strangulation elsewhere.

When large, and complicating pregnancy and delivery, the results may be serious, though a fatal result is very rare.

Dr. Barker relates three cases which came under his own observation, all of great interest, besides a fourth which was communicated to him.

3. Professor von Nussbaum describes a most instructive and interesting case of ovariectomy which was followed by an abdominal urinary fistula, to obviate which, he undertook an operation for the formation of an artificial ureter. The plan he adopted was as follows:—Having determined by coloured injections that there was no communication between the abdominal urinary fistula and the bladder, he passed one finger of the right into the bladder, and one finger of the left hand into the abdominal opening, endeavouring to make them meet. Failing in this, he found that he could feel with the left finger the point of a catheter passed into the bladder and pushed upwards towards the abdomen. He then thrust a trocar in this direction, which he judged to correspond pretty nearly with that of the normal course of the ureter. Through the channel thus made he passed an india rubber drainage tube, into which was fitted a glass tube furnished with a flange, which remained in the bladder and prevented upward movement of the tubes. For a short time this succeeded, but in spite of the flange the tubes escaped upwards, and

urine again flowed through the abdominal wound. A large glass tube was inserted, but that also ultimately failed to keep in the proper place. Eventually, a sufficient opening was formed for the downward flow of the urine, and ultimately it took only this channel, the abdominal opening closing after some weeks.

Nussbaum preferred this plan to removing the kidney (as Simon successfully did in a similar case), or to obliterating the ureter with a view to causing atrophy of the kidney. Both were dangerous, the latter especially so.

4. The subject of membranous dysmenorrhœa has long been involved in difficulty, partly owing to the designations applied to it, and partly to the rarity with which accurate microscopic examination of the substances expelled have been made by competent observers. Dr. Beigel, whose competency in every respect no one can gainsay, regards the affection as an exfoliative endometritis. It is essentially characterised (*a*) by the expulsion of a membrane at the time of menstruation, and (*b*) by the expulsion of this membrane at indeterminate intervals or recurring every month.

Hence this exfoliative endometritis may exist, and does exist without dysmenorrhœa; physiology, etiology, and pathological anatomy demonstrate its nature. The name membranous dysmenorrhœa then is bad. At the same time the membrane expelled is not the consequence of a very early abortion; an erroneous idea which the name decidua menstrualis might justify. On the contrary, this affection is a cause of sterility or abortion. Moreover, it not rarely occurs in virgins, and its monthly expulsion has been observed in many cases.

Beigel's opinion is based on etiology and pathological anatomy. The affection is not specific; it comes on after primitive or secondary endometritis; it is often connected with diseases of very varied nature; general diseases, such as cholera and phthisis; uterine diseases, such as flexions, and especially retroversion; chronic metritis, tumours, particularly fibroids and polypi, and mechanical irritation. The formation of a membrane is the principal anatomico-pathological characteristic of the disorder. Its expulsion is but a consequence, an accident, which attends menstruation and metrorrhagias, and uterine contractions, which coincide with them. As the causes are various, so the microscopical characters of the expelled membranes are different. They all, however, present a common character; they are separated from the subjacent mucous membrane by a fibrinous layer containing free round cells. They are constituted sometimes by hyperplasia of the normal elements of the mucous membrane, sometimes by degeneration, by destruction of its parts, glands and epithelium; sometimes by the production of embryonal elements, and lastly, sometimes they contain round cells, flattened epithelium, and embryonal elements at the same time.

5. Professor Kœberlé states that it is often possible to distinguish ovarian cysts from other serous sub-peritoneal cysts by the nature of the fluid they contain. This diagnosis rests principally on the presence or absence of albumen, metalbumen, and paralbumen. Par-

ovarian cysts, or those of the broad ligament, contain a very fluid liquid, generally colourless and limpid as water, sometimes quite salt, but generally not containing any albuminous material; when it does contain albumen it is the paralbumen, which is precipitated by nitric acid, but the precipitate redissolves in acetic acid.

Cysts of the fallopian tube contain only albumen and no paralbumen; the precipitate produced by nitric acid is increased by acetic acid.

Ovarian cysts furnish a liquid charged with albumen, metalbumen, and especially paralbumen, giving a precipitate soluble in nitric acid.

Well marked, these reactions are conclusive; but there are exceptional cases where they are but feebly present; Kœberlé has even seen the fluid of parovarian cysts containing traces of albumen, and inversely that of ovarian cysts contain so little albumen and paralbumen, that one could hardly think the cyst ovarian.

All doubt is removed, however, and we have to do with an ovarian cyst when puncture gives exit to a glutinous fluid, sometimes entirely uncoagulable by heat and nitric acid, rarely limpid, containing only traces of albumen (colloid cysts), or a fatty liquid containing in suspension mucus and epidermic detritus or hair (dermoid cysts). Lastly, examined by the microscope, the fluid of ovarian cysts contains granular globules, yellowish, 0·003 millimètres to 0·060 millimètres in diameter, the envelope being rendered more apparent by acetic or phosphoric acids.

To the foregoing signs the following may be added:—In sub-peritoneal serous cysts there is no loss of flesh. The tumour, unilocular, presents a very manifest fluctuation; its walls are thin; its development slow, though at times rapid enough; it sometimes attains a considerable size without becoming adherent to neighbouring organs; sometimes it is small and very adherent.

In cysts of the ovary wasting is pronounced. The tumour, whether uni- or multi-locular, often presents a limited fluctuation; its walls are occasionally thin, occasionally more or less thick and resistant, hard and nodulated; its development is ordinarily rapid, sometimes slow; lastly, it is adherent whenever the volume is at all considerable.

6. Dr. Fabbri relates a case in which both the vagina and the uterus were double. There was atresia of one of the vaginæ and a collection of blood in the uterus of the same side. The presence of a swelling and fluctuation could be made out through the permeable vagina. The catamenia had been painful and accompanied by colic.

An opening was made into the closed vagina, and the contents having been evacuated the patient recovered. It was then found that there was a septum between the two canals, and that to each vagina corresponded a cervix and corpus uteri; there was, in other word, incomplete fusion of the two canals of Müller.

7. In an Inaugural Dissertation (Halle) Dr. Kryzan relates an interesting case of rupture of an ovarian cyst, the connexions of which were ultimately verified by post-mortem examination. The

patient, aged thirty-six, about a year after her fifth confinement presented all the signs of a multilocular cyst of the ovary. She was suddenly seized without any appreciable cause with abdominal pains, vomiting, dyspnoea and collapse. The cyst was found to have diminished in volume but there was evidence of free fluid in the peritoneal cavity and the fluid pushed down the posterior wall of the vagina. A puncture was made at that point and a large quantity of yellowish serous fluid was evacuated.

The patient recovered, but some time after, owing to a fall, analogous symptoms recurred and puncture by the abdominal wall gave exit to a colloid fluid strongly mixed with blood. Death ensued and at the autopsy the abdominal cavity was found to be filled with similar fluid. The cyst developed in the broad ligament showed an adhesion to the spleen, which had become torn, and hence the blood. The cyst was found to be partly glandular and partly papillomatous.

8. Dr. Kuster's case is of considerable interest. The patient, fifty-seven years old, had a large abdomen, there was a small tubercle on the summit of the navel and fungous excrescences at the sides and towards the sternum.

A malignant growth of the ovaries with secondary degeneration of the umbilicus and sternum was diagnosed. Ovariectomy, which was urgently desired by the patient, was in consequence refused; and only occasional punctures were made giving exit to coffee coloured fluid containing albumen and colloid corpuscles.

After tapping a very voluminous solid tumour could be made out. The patient died of exhaustion. Post-mortem a tumour strongly adherent to the periosteum of the sternum; another occupying the right side of the umbilical ring and between the peritoneum and the skin; and four abdominal tumours were found. The latter were attached respectively to the left ovary, the left Fallopian tube and the uterus, and behind these parts a large mass filled Douglas's pouch.

9. This case illustrates the dangers attending rectal exploration after Simon's method, as well as an unusual development of cancer.

The tumours which existed in this case declared themselves during the course of the sixteenth pregnancy and were regarded as benign growths of the ovary.

The patient was delivered without difficulty and suckled her child. After some time the patient wished for an operation to be done to rid her of the tumours. Spiegelberg introduced the whole hand into the rectum, but could glean no information from it as to the seat and relations of the tumour, but as he withdrew his hand a quantity of clear brown serous fluid escaped. More followed as he repeated the exploration. As there had previously been ascites, and as the abdomen decreased and the tumour become more perceptible to palpation, it was evident that the abdominal cavity had been perforated through the rectum. The patient died of peritonitis which was found post-mortem, together with much gas and pus, cancerous degeneration of both ovaries, and a perforation at the pyloric end of the stomach at a point where a cancerous nodule growth was seated. The rectal perforation is not mentioned.

10. Dr. Palasciano recounts a case in which he successfully ablated the body of the uterus in an old-standing case of inversion.

Loops of iron wire were tightened around the cervix and the body was amputated by the scissors. No blood was lost, and the patient rapidly recovered. The ovaries and vagina were left unimpaired, and imperfect attempts at menstruation have ensued, small quantities of blood escaping periodically.

11. Dr. Thomas gives the particulars of five cases, in four of which he operated most successfully for the enucleation of the fibroid growths. The capsule is incised if necessary, and ergot is given to cause the extension of the growths, which are then dealt with *sec. art.*

Dr. Thomas does not omit to give a just meed of praise to Dr. W. L. Atlee, to whom the credit of originating this method of practice is due.

Dr. Atlee proposed it in an essay for which he was awarded a prize by the American Medical Association in 1853, and which "to-day stands as the pioneer article in the surgical literature of these grave and often irremediable cases."

#### DISEASES OF CHILDREN.

1. *On Tubercular Meningitis in Children.* By Dr. BERTALOT ('Jahrb. f. Kinderheilk.,' Band ix, heft 3).
2. *Peri-nephritic Abscess in Children.* By Dr. GIBNEY ('Amer. Journ. of Obstetrics,' April, 1876).
3. *On a certain Tumour of the Neck in newly-born Children.* By Drs. BLACHEZ, PLANTEAU, and PERATÉ ('Gaz. Hebdom.,' May 19 and 26, 1876), and Dr. WINSLOW ('Philadel. Med. Times,' Feb., 1876).
4. *On the recurrence of Scarlet Fever.* By Dr. KÖRNER ('Jahrb. f. Kinderheilk.,' Bd. ix, April, 1876).
5. *Acute Atrophy of the Liver in a Child.* By Drs. REHN and PERLS ('Berlin Klin. Wochensch.,' Nov., 1875).
6. *A case of Sudden Amaurosis associated with Chorea.* By Dr. FITZGERALD ('Dub. Journ. of Med. Sci.,' March, 1876).
7. *A case of congenital Striated Myo-sarcoma of the Kidneys.* By Dr. COHNHEIM ('Arch. f. Path. Anät. Med. Phys.,' Bd. xxv, p. 64).
8. *A case of Hemorrhage into the Ventricles of the Brain in a child six months old.* By Dr. DULLES ('Philadelphia Med. Times,' July, 1876).

1. Dr. Bertalot gives an account of twenty-four (24) cases of tubercular meningitis occurring in children, in all of which a post-mortem examination was made.

The age of the patients varied from ten weeks to fourteen years. The symptoms observed by Dr. Bertalot were in accord with those now commonly regarded as classical. At the onset there was wasting pretty constantly. This often spared the face. Capricious



appetite, dulness, constipation, sleepiness, headache, nightmare, and grinding of the teeth were frequently seen. Later on vomiting was an almost constant symptom, being absent only in two cases. It was of a peculiar character, and was generally effected without exertion. There was constipation as a rule, and generally retraction of the belly.

Fever was a constant symptom, but the temperature never exceeded  $103.1^{\circ}$  F.

Flushing of the face was generally marked, and so was cerebral breathing. Headache was commonly a marked symptom.

Convulsions, partial or general, were observed. Squinting was pretty constant, and so was spastic rigidity of various parts.

Though Dr. Bertalot used the ophthalmoscope, he states that he did not find tubercle in the choroid nor enlargement of the papilla. Fluid was found distending the ventricles in all cases.

2. The symptoms in Dr. Gibney's cases closely resembled those of acute hip-joint disease; but subsequently tumefaction and fluctuation in the loin or groin came on. None of the cases ended fatally, and some recovered without the abscess being opened. There might be difficulty in making the differential diagnosis, and especially between peri-nephritic abscess and acute spinal caries.

3. MM. Blachez, Planteau, and Peraté record their experience of a peculiar affection observed in newly-born children, and Dr. Winslow relates a further instance of the disease.

The children present a hard, elastic, ovoid tumour, in the thickness of the sterno-mastoid muscle of the right side, which has not been noticed in these cases until several days after birth. The skin was normal and without deep adhesions; the tumour appeared limited to the muscle and situated especially in the sternal bundle. Except when the head was moved there was but little pain. The head inclined towards the affected side, and could only be restored to the normal position incompletely.

The cause of these tumours could not be connected with syphilis, but the coincidence, in all the cases of MM. Blachez, Planteau and Peraté, of presentation by the breech, led those gentlemen to adopt the following explanation:—

The tumour is probably the result of traction on the muscle during the extraction of the head; and what confirms this view is that the tumour was situated on the right side in each case. There is produced through the influence of this traction, which hardly causes rupture of the muscle, an interstitial myositis which gradually increases, and this accounts for the slow appearance of the tumour after birth. Resolution takes place at the end of two or three months; frictions with mercurial ointment and extract of belladonna constituting the only treatment. The syphilitic nature of the disease must not be inferred from this treatment, for any suspicion of that diathesis was eliminated by the most minute inquiry.

Dr. Winslow's case differs somewhat from the foregoing, inasmuch as the manner of birth was perfectly normal, and the tumour

was situate on the left side. It was as hard as cartilage, movable on the deeper tissues, and without any symptom of inflammation. No treatment was adopted, and the mass disappeared spontaneously towards the end of the third month. There was no evidence of syphilis. Dr. Ashurst remarked on this case that traumatism and congenital syphilis were the two causes of such tumours; and Dr. Horace Williams stated that he had met with two cases, and in both there was a breech presentation, confirming the French observers.

4. Dr. Kœrner has made some researches on the subject of the recurrence of scarlet fever. A few writers deny that it recurs, but the majority are of opinion that it is a disease which is prone to do so. A distinction is drawn between relapse (before desquamation), recurrence (during desquamation), and second attacks after the lapse of a considerable time. The two first are not without gravity, though not often fatal, but the third is not more benign than a first attack.

5. The child was only two and a half years old. At the onset there were jaundice, diarrhoea, and want of appetite. The urine appeared to contain bile and the motions were grey. A rapid diminution of the volume of the liver ensued, and the patient died nine days after the first appearance of jaundice. The urine drawn from the bladder after death contained neither leucine, tyrosine, nor bile acids, but a quantity of pigment. No trace of bile was found in the bile-ducts or gall-bladder. The liver was small, and under the microscope hepatic cells were barely recognisable; there were many crystals of bilirubine in the parts that were fattily degenerated and in the parts that were of a red colour. There was fatty degeneration of the kidneys.

6. The case narrated by Dr. Fitzgerald is of singular interest in reference to certain views respecting the pathology of chorea. A child, ten years of age, complained one evening of headache. The next day she said she could not see with the left eye. Shortly after she was seized with chorea, chiefly affecting the left side. Ophthalmoscopic examination showed embolism of the central artery of the retina; gradually vision was restored. The treatment consisted of bromide and iodide of potassium. Subsequently the patient was suddenly seized with facial paralysis of the right side and cerebral vomiting, which renders probable the hypothesis of a profound lesion of the nervous centres.

7. A child, a year old and quite healthy, was found to have a small tumour in the left loin. This rapidly enlarged, and at the same time the health failed seriously. At the end of three months from its first appearance the child died, the tumour having completely filled the abdominal cavity. The urine had become suppressed towards the end of life, but it had never been albuminous. The tumour, of an ovoid form, was found to spring from the left kidney, and it was entirely covered by a thick fibrous capsule. The right kidney was normal, with the exception of a small spot, which showed an alteration identical with the growth affecting the left

kidney. On section, the tumour was found to be composed of a series of nodules resembling in consistence that of a medullary osteo-sarcoma, or a huge fibro-myoma of the uterus.

Cohnheim was surprised to find these nodules to be, for the most part, composed of perfectly characteristic striated muscular fibres. It was impossible to discover a sarcolemma. In other nodules the typical structure of a sarcoma was occasionally discovered. The line of demarcation between the sound tissue of the kidney and that of the growth was not well marked either to the microscope or to the naked eye.

Owing to the presence in the other kidney of a similar though infinitely smaller growth, Cohnheim regards the case as one of a primitive vice of formation and not one of metastasis.

8. This case was that of a child who was deserted by her mother, but there is no evidence of violence having been resorted to. The child had pneumonia, for which she was treated for some days. No symptoms suggestive of cerebral mischief were noted.

*Post-mortem.*—The liver and kidneys were found to be fatty, and there was pulmonary consolidation. The membranes of the brain were congested but not inflamed. The lateral ventricles were filled with firm and partly organized clots, entangled with the choroid plexus of each side. The third and sixth ventricles were also filled with clots. The brain substance about each ventricle was deeply stained. The origin of the hæmorrhage could not be found. Such cases are extremely rare, only ten cases of true cerebral hæmorrhage in children, *i.e.*, hæmorrhage into the substance of the brain as distinguished from hæmorrhage between the membranes, being on record (Trousseau, Niemeyer, West, Meigs and Pepper, Rilliet and Barthex).

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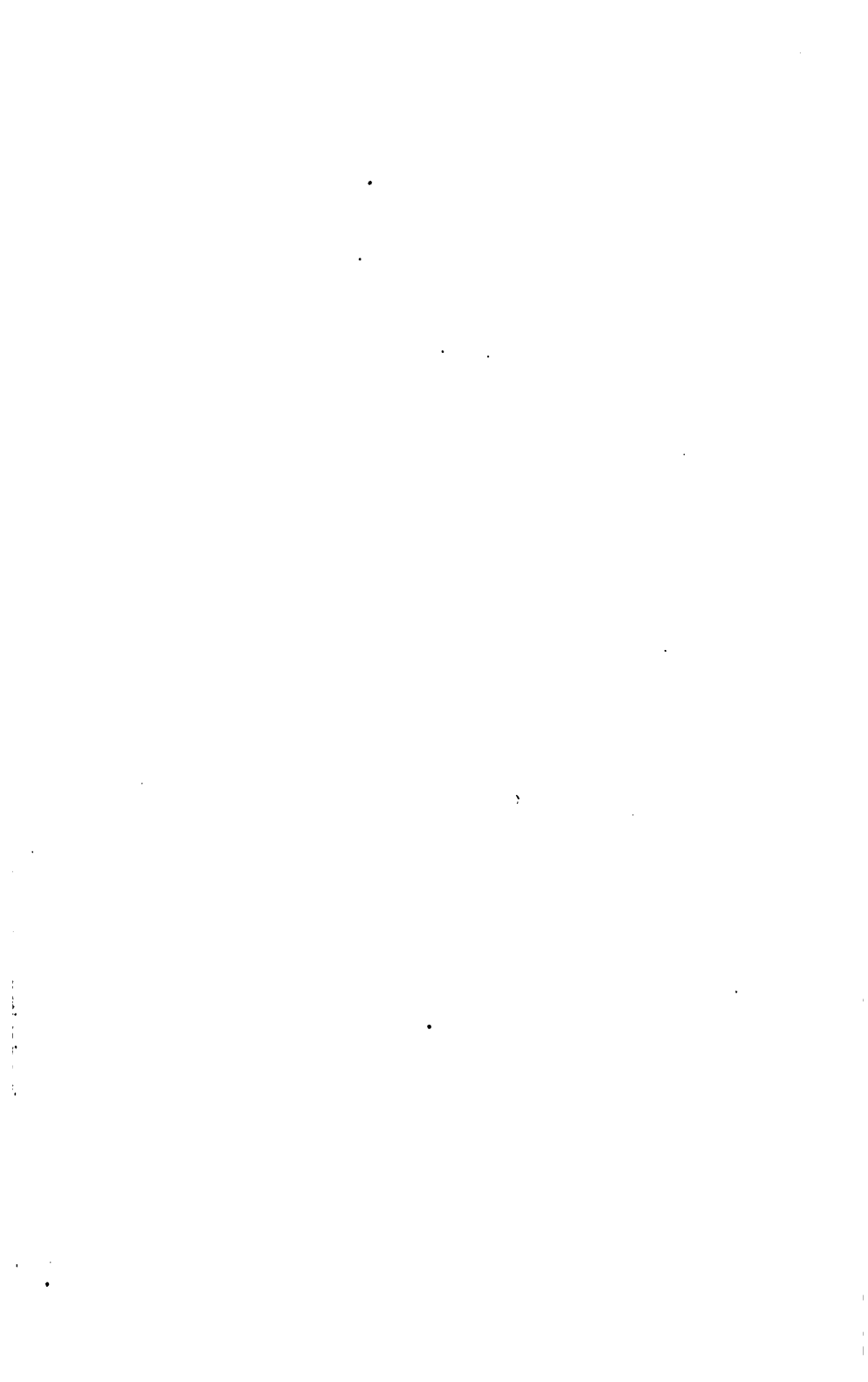
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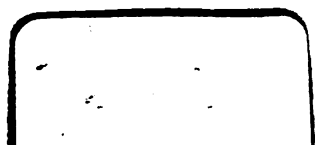
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